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EXPERIMENTAL RESEARCHES
IN
ELECTRICITY.

VOL. II.

EXPERIMENTAL RESEARCHES

IN

ELECTRICITY.

BY

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VOL. II.

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PREFACE.

For reasons stated in the former volume of Experimental Researches in Electricity, I have been induced to gather the remaining Series together, and to add to them certain other papers devoted to Electrical research.

To the prefatory remarks containing these reasons, I would recall the recollection of those who may honour these Researches with any further attention. I have printed the papers in this volume, as before, with little or no alteration, except that I have placed the fair and just date of each at the top of the pages.

I regret the presence of those papers which partake of a controversial character, but could not help it; some of them contain much new, important and explanatory matter. The introduction of matter due to other parties than myself, as Nobili and Antinori, or Hare, was essential to the comprehension of the further development given in the replies.

I owe many thanks to the Royal Society, to Mr. Murray, and to Mr. Taylor, for the great kindness I have received in the loan of plates, &c., and in other facilities granted to me for the printing of the volume.

As the Index belongs both to the Experimental Researches and to the miscellaneous papers, its references are of necessity made in two ways; those to the Researches are, as before, to the numbers of the Paragraphs, and are easily recognised by the greatness of the numbers: the other references are to the pages, and being always preceded by *p.* or *pp.*, are known by that mark.

MICHAEL FARADAY.

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EXPERIMENTAL RESEARCHES
IN
ELECTRICITY.

FIFTEENTH SERIES.

§ 23. *Notice of the character and direction of the electric force of the Gymnotus.*

Received November 15,—Read December 6, 1838.

1749. WONDERFUL as are the laws and phenomena of electricity when made evident to us in inorganic or dead matter, their interest can bear scarcely any comparison with that which attaches to the same force when connected with the nervous system and with life; and though the obscurity which for the present surrounds the subject may for the time also veil its importance, every advance in our knowledge of this mighty power in relation to inert things, helps to dissipate that obscurity, and to set forth more prominently the surpassing interest of this very high branch of Physical Philosophy. We are indeed but upon the threshold of what we may, without presumption, believe man is permitted to know of this matter; and the many eminent philosophers who have assisted in making this subject known have, as is very evident in their writings, felt up to the latest moment that such is the case.

1750. The existence of animals able to give the same concussion to the living system as the electrical machine, the voltaic battery, and the thunder storm, being with their habits made known to us by Richer, S'Gravesende, Firmin, Walsh, Humboldt, &c. &c., it became of growing importance to identify the living power which they possess, with that which man can call into action from inert matter, and by him named electricity (265. 351.). With the *Torpedo* this has been done to perfection, and the direction of the current of force determined by the united and successive labours of Walsh[1], Cavendish[2], Galvani[3], Gardini[4], Humboldt and Gay-Lussac[5], Todd[6], Sir Humphry Davy[7], Dr. Davy[8], Becquerel[9], and Matteucci[10].

1751. The Gymnotus has also been experimented with for the same purpose, and the investigations of Williamson[11], Garden[12], Humboldt[13], Fahlberg[14], and Guisan[15], have gone very far in showing the identity of the electric force in this animal with the electricity excited by ordinary means; and the two latter philosophers have even obtained the spark.

1752. As an animal fitted for the further investigation of this refined branch of science, the Gymnotus seems, in certain respects, better adapted than the Torpedo, especially (as Humboldt has remarked) in its power of bearing confinement, and capability of being preserved alive and in health for a long period. A Gymnotus has been kept for several months in activity, whereas Dr. Davy could not preserve Torpedos above twelve or fifteen days; and Matteucci was not able out of 116 such fish to keep one living above three days, though every circumstance favourable to their preservation was attended to[16]. To obtain Gymnoti has therefore been a matter of consequence; and being stimulated, as much as I was honoured, by very kind communications from Baron Humboldt, I in the year 1835 applied to the Colonial Office, where I was promised every assistance in procuring some of these fishes, and continually expect to receive either news of them or the animals themselves.

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[1] Philosophical Transactions, 1773, p. 461.
[2] Ibid. 1776, p. 196.
[3] Aldini's Essai sur la Galvanism, ii. 61.
[4] De Electrici ignis Natura, §. 71. Mantua, 1792.
[5] Annales de Chimie, xiv. 15.
[6] Philosophical Transactions, 1816, p. 120.
[7] Ibid. 1829, p. 15.
[8] Ibid. 1832, p. 259; and 1834, p. 531.
[9] Traité de l'E'lectricité, iv. 264.
[10] Bibliothèque Universelle, 1837, tom. xii. 163.
[11] Philosophical Transactions, 1775, p. 94.
[12] Ibid. 1775, p. 102.
[13] Personal Narrative, chap. xvii.
[14] Swedish Transactions, 1801, pp. 122. 156.
[15] De Gymnoto Electrico. Tubingen, 1819.
[16] Bibliothèque Universelle, 1837, xii. p. 174.

1753. Since that time Sir Everard Home has also moved a friend to send some Gymnoti over, which are to be consigned to His Royal Highness our late President; and other gentlemen are also engaged in the same work. This spirit induces me to insert in the present communication that part of the letter from Baron Humboldt which I received as an answer to my inquiry of how they were best to be conveyed across the Atlantic. He says, "The Gymnotus, which is common in the Llanos de Caracas (near Calabozo), in all the small rivers which flow into the Orinoco, in English, French or Dutch Guiana, is not of difficult transportation. We lost them so soon at Paris because they were too much fatigued (by experiments) immediately after their arrival. MM. Norderling and Fahlberg retained them alive at Paris above four months. I would advise that they be transported from Surinam (from Essequibo, Demerara, Cayenne) in summer, for the Gymnotus in its native country lives in water of 25° centigrade (or 77° Fahr.). Some are five feet in height, but I would advise that such as are about twenty-seven or twenty-eight inches in length be chosen. Their power varies with their food, and their state of rest. Having but a small stomach they eat little and often, their food being cooked meat, *not salted*, small fish, or even bread. Trial should be made of their strength and the fit kind of nourishment before they are shipped, and those fish only selected already accustomed to their prison. I retained them in a box or trough about four feet long, and sixteen inches wide and deep. The water must be *fresh*, and be changed every three or four days: the fish must not be prevented from coming to the surface, for they like to swallow air. A net should be put over and round the trough, for the Gymnotus often springs out of the water. These are all the directions that I can give you. It is, however, *important* that the animal should not be tormented or fatigued, for it becomes exhausted by frequent electric explosions. Several Gymnoti may be retained in the same trough."

1754. A Gymnotus has lately been brought to this country by Mr. Porter, and purchased by the proprietors of the Gallery in Adelaide Street: they immediately most liberally offered me the liberty of experimenting with the fish for scientific purposes; they placed it for the time exclusively at my disposal, that (in accordance with Humboldt's directions (1753.)) its powers might not be impaired; only desiring me to have a regard for its life and health. I was not slow to take advantage of their wish to forward the interests of science, and with many thanks accepted their offer. With this Gymnotus, having the kind assistance of Mr. Bradley of the Gallery, Mr. Gassiot, and occasionally other gentlemen, as Professors Daniell, Owen and Wheatstone, I have obtained every proof of the identity of its power with common electricity (265. 351, &c.). All of these had been obtained before with the Torpedo (1750.), and some, as the shock, circuit, and spark (1751.), with the Gymnotus; but still I think a brief account of the results will be acceptable to the Royal Society, and I give them as necessary preliminary experiments to the investigations which we may hope to institute when the expected supply of animals arrives (1752.).

1755. The fish is forty inches long. It was caught about March 1838; was brought to the Gallery on the 15th of August, but did not feed from the time of its capture up to the 19th of October. From the 24th of August Mr. Bradley nightly put some blood into the water, which was changed for fresh water next morning, and in this way the animal perhaps obtained some nourishment. On the 19th of October it killed and eat four small fish; since then the blood has been discontinued, and the animal has been improving ever since, consuming upon an average one fish daily[1].

1756. I first experimented with it on the 3rd of September, when it was apparently languid, but gave strong shocks when the hands were favourably disposed on the body (1760. 1773, &c.). The experiments were made on four different days, allowing periods of rest from a month to a week between each. His health seemed to improve continually, and it was during this period, between the third and fourth days of experiment, that he began to eat.

1757. Beside the hands two kinds of collectors were used. The one sort consisted each of a copper rod fifteen inches long, having a copper disc one inch and a half in diameter brazed to one extremity, and a copper cylinder to serve as a handle, with large contact to the hand, fixed to the other, the rod from the disc upwards being well covered with a thick caoutchouc tube to insulate that part from the water. By these the states of particular parts of the fish whilst in the water could be ascertained.

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[1] The fish eaten were gudgeons, carp, and perch.

1758. The other kind of collectors were intended to meet the difficulty presented by the complete immersion of the fish in water; for even when obtaining the spark itself I did not think myself justified in asking for the removal of the animal into air. A plate of copper eight inches long by two inches and a half wide, was bent into a saddle shape, that it might pass over the fish, and inclose a certain extent of the back and sides, and a thick copper wire was brazed to it, to convey the electric force to the experimental apparatus; a jacket of sheet caoutchouc was put over the saddle, the edges projecting at the bottom and the ends; the ends were made to converge so as to fit in some degree the body of the fish, and the bottom edges were made to spring against any horizontal surface on which the saddles were placed. The part of the wire liable to be in the water was covered with caoutchouc.

1759. These conductors being put over the fish, collected power sufficient to produce many electric effects; but when, as in obtaining the spark, every possible advantage was needful, then glass plates were placed at the bottom of the water, and the fish being over them, the conductors were put over it until the lower caoutchouc edges rested on the glass, so that the part of the animal within the caoutchouc was thus almost as well insulated as if the Gymnotus had been in the air.

1760. *Shock.*—The shock of this animal was very powerful when the hands were placed in a favourable position, i. e. one on the body near the head, and the other near the tail; the nearer the hands were together within certain limits the less powerful was the shock. The disc conductors (1757.) conveyed the shock very well when the hands were wetted and applied in close contact with the cylindrical handles; but scarcely at all if the handles were held in the dry hands in an ordinary way.

1761. *Galvanometer.*—Using the saddle conductors (1758.) applied to the anterior and posterior parts of the Gymnotus, a galvanometer was readily affected. It was not particularly delicate; for zinc and platina plates on the upper and lower surface of the tongue did not cause a permanent deflection of more than 25°; yet when the fish gave a powerful discharge the deflection was as much as 30°, and in one case even 40°. The deflection was constantly in a given direction, the electric current being always from the anterior parts of the animal through the galvanometer wire to the posterior parts. The former were therefore for the time externally positive, and the latter negative.

1762. *Making a magnet.*—When a little helix containing twenty-two feet of silked wire wound on a quill was put into the circuit, and an annealed steel needle placed in the helix, the needle became a magnet, and the direction of its polarity in every case indicated a current from the anterior to the posterior parts of the Gymnotus through the conductors used.

1763. *Chemical decomposition.*—Polar decomposition of a solution of iodide of potassium was easily obtained. Three or four folds of paper moistened in the solution (322.) were placed between a platina plate and the end of a wire also of platina, these being respectively connected with the two saddle conductors (1758.). Whenever the wire was in conjunction with the conductor at the fore part of the Gymnotus, iodine appeared at its extremity; but when connected with the other conductor, none was evolved at the place on the paper where it before appeared. So that here again the direction of the current proved to be the same as that given by the former tests.

1764. By this test I compared the middle part of the fish with other portions before and behind it, and found that the conductor A, which being applied to the middle was negative to the conductor B applied to the anterior parts, was, on the contrary, positive to it when B was applied to places near the tail. So that within certain limits the condition of the fish externally at the time of the shock appears to be such, that any given part is negative to other parts anterior to it, and positive to such as are behind it.

1765. *Evolution of heat.*—Using a Harris's thermo-electrometer belonging to Mr. Gassiot, we thought we were able in one case, namely, that when the deflection of the galvanometer was

40° (1761.), to observe a feeble elevation of temperature. I was not observing the instrument myself, and one of those who at first believed they saw the effect now doubts the result[1].

1766. *Spark.*—The electric spark was obtained thus. A good magneto-electric coil, with a core of soft iron wire, had one extremity made fast to the end of one of the saddle collectors (1758.), and the other fixed to a new steel file; another file was made fast to the end of the other collector. One person then rubbed the point of one of these files over the face of the other, whilst another person put the collectors over the fish, and endeavoured to excite it to action. By the friction of the files contact was made and broken very frequently; and the object was to catch the moment of the current through the wire and helix, and by breaking contact *during the current* to make the electricity sensible as a spark.

1767. The spark was obtained four times, and nearly all who were present saw it. That it was not due to the mere attrition of the two piles was shown by its not occurring when the files were rubbed together, independently of the animal. Since then I have substituted for the lower file a revolving steel plate, cut file fashion on its face, and for the upper file wires of iron, copper and silver, with all of which the spark was obtained[2].

1768. Such were the general electric phenomena obtained from this Gymnotus whilst living and active in his native element. On several occasions many of them were obtained together; thus a magnet was made, the galvanometer deflected, and perhaps a wire heated, by one single discharge of the electric force of the animal.

1769. I think a few further but brief details of experiments relating to the quantity and disposition of the electricity in and about this wonderful animal will not be out of place in this short account of its powers.

1770. When the shock is strong, it is like that of a large

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[1] In more recent experiments of the same kind we could not obtain the effect.

[2] At a later meeting, at which attempts were made to cause the attraction of gold leaves, the spark was obtained directly between fixed surfaces, the inductive coil (1766.) being removed, and only short wires (by comparison) employed.

Leyden battery charged to a low degree, or that of a good voltaic battery of perhaps one hundred or more pair of plates, of which the circuit is completed for a moment only. I endeavoured to form some idea of the *quantity* of electricity by connecting a large Leyden battery (291.) with two brass balls, above three inches in diameter, placed seven inches apart in a tub of water, so that they might represent the parts of the Gymnotus to which the collectors had been applied; but to lower the intensity of the discharge, eight inches in length of six-fold thick wetted string were interposed elsewhere in the circuit, this being found necessary to prevent the easy occurrence of the spark at the ends of the collectors (1758.), when they were applied in the water near to the balls, as they had been before to the fish. Being thus arranged, when the battery was strongly charged and discharged, and the hands put into the water near the balls, a shock was felt, much resembling that from the fish; and though the experiments have no pretension to accuracy, yet as the tension could be in some degree imitated by reference to the more or less ready production of a spark, and after that the shock be used to indicate whether the quantity was about the same, I think we may conclude that a single medium discharge of the fish is at least equal to the electricity of a Leyden battery of fifteen jars, containing 3500 square inches of glass coated on both sides, charged to its highest degree (291.). This conclusion respecting the great quantity of electricity in a single Gymnotus shock, is in perfect accordance with the degree of deflection which it can produce in a galvanometer needle (367. 860. 1761.), and also with the amount of chemical decomposition produced (374. 860. 1763.) in the electrolyzing experiments.

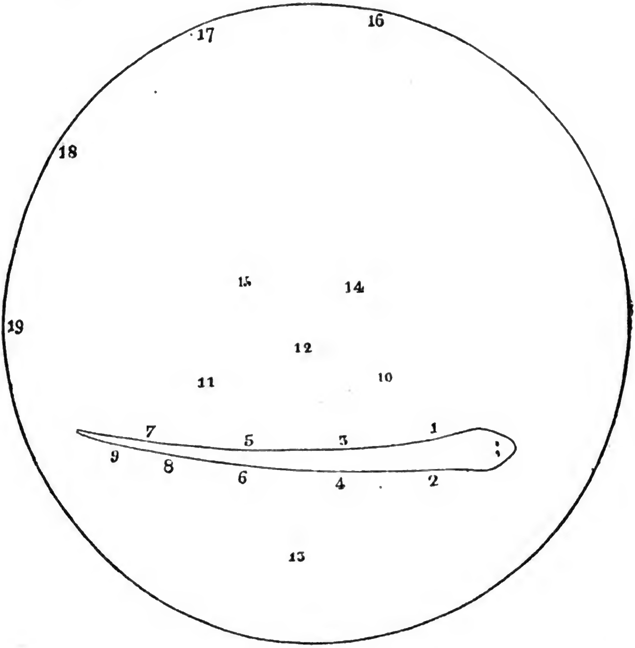
1771. Great as is the force in a single discharge, the Gymnotus, as Humboldt describes, and as I have frequently experienced, gives a double and even a triple shock; and this capability of immediately repeating the effect with scarcely a sensible interval of time, is very important in the considerations which must arise hereafter respecting the origin and excitement of the power in the animal. Walsh, Humboldt, Gay-Lussac, and Matteucci have remarked the same thing of the Torpedo, but in a far more striking degree.

1772. As, at the moment when the fish wills the shock, the anterior parts are positive and the posterior parts negative, it may be concluded that there is a current from the former to the latter through every part of the water which surrounds the animal, to a considerable distance from its body. The shock which is felt, therefore, when the hands are in the most favourable position, is the effect of a very small portion only of the electricity which the animal discharges at the moment, by far the largest portion passing through the surrounding water. This enormous external current must be accompanied by some effect within the fish *equivalent* to a current, the direction of which is from the tail towards the head, and equal to the sum of *all these external forces.* Whether the process of evolving or exciting the electricity within the fish includes the production of this internal current (which need not of necessity be as quick and momentary as the external one), we cannot at present say; but at the time of the shock the animal does not apparently feel the electric sensation which he causes in those around him.

1773. By the help of the accompanying diagram I will state a few experimental results which illustrate the current around the fish, and show the cause of the difference in character of the shock occasioned by the various ways in which the person is connected with the animal, or his position altered with respect to it. The large circle represents the tub in which the animal is confined; its diameter is forty-six inches, and the depth of water in it three inches and a half; it is supported on dry wooden legs. The figures represent the places where the hands or the disc conductors (1757.) were applied, and where they are close to the figure of the animal, it implies that contact with the fish was made. I will designate different persons by A, B, C, &c., A being the person who excited the fish to action.

1774. When one hand was in the water the shock was felt in that hand only, whatever part of the fish it was applied to; it was not very strong, and was only in the part immersed in the water. When the hand and part of the arm was in, the shock was felt in all the parts immersed.

1775. When *both* hands were in the water at the *same* part of the fish, still the shock was comparatively weak, and only in the parts immersed. If the hands were on opposite sides, as at 1, 2, or at 3, 4, or 5, 6, or if one was above and the other below at the same part, the effect was the same. When the disc collectors were used in these positions no effect was felt by the person holding them (and this corresponds with the observation of Gay-Lussac on Torpedos[1]), whilst other persons, with both hands in at a distance from the fish, felt considerable shocks.



1776. When both hands or the disc collectors were applied at places separated by a part of the length of the animal, as at 1, 3, or 4, 6, or 3, 6, then strong shocks extending up the arms, and even to the breast of the experimenter, occurred, though another person with a single hand in at any of these places, felt comparatively little. The shock could be obtained at parts very near the tail, as at 8, 9. I think it was strongest at about 1 and 8. As the hands were brought nearer together the effect diminished, until being in the same cross plane, it was, as before described, only sensible in the parts immersed (1775.).

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[1] Annales de Chimie, xiv. p. 18.

1777. B placed his hands at 10, 11, at least four inches from the fish, whilst A touched the animal with a glass rod to excite it to action; B quickly received a powerful shock. In another experiment of a similar kind, as respects the non-necessity of touching the fish, several persons received shocks independently of each other; thus A was at 4, 6; B at 10, 11; C at 16, 17; and D at 18, 19; all were shocked at once, A and B very strongly, C and D feebly. It is very useful, whilst experimenting with the galvanometer or other instrumental arrangements, for one person to keep his hands in the water at a moderate distance from the animal, that he may know and give information when a discharge has taken place.

1778. When B had both hands at 10, 11, or at 14, 15, whilst A had but one hand at 1, or 3, or 6, the former felt a strong shock, whilst the latter had but a weak one, though in contact with the fish. Or if A had both hands in at 1, 2, or 3, 4, or 5, 6, the effect was the same.

1779. If A had the hands at 3, 5, B at 14, 15, and C at 16, 17, A received the most powerful shock, B the next powerful, and C the feeblest.

1780. When A excited the Gymnotus by his hands at 8, 9, whilst B was at 10, 11, the latter had a much stronger shock than the former, though the former touched and excited the animal.

1781. A excited the fish by one hand at 3, whilst B had both hands at 10, 11 (or along), and C had the hands at 12, 13 (or across); A had the pricking shock in the immersed hand only (1774.); B had a strong shock up the arms; C felt but a slight effect in the immersed parts.

1782. The experiments I have just described are of such a nature as to require many repetitions before the general results drawn from them can be considered as established; nor do I pretend to say that they are anything more than indications of the direction of the force. It is not at all impossible that the fish may have the power of throwing each of its four electric organs separately into action, and so to a certain degree direct the shock, i. e. he may have the capability of causing the electric current to emanate from one side, and at the same time bring the other side of his body into such a condition, that it shall be as a non-conductor in that direction. But I think the appearances and results are such as to forbid the supposition, that he has any control over the direction of the currents after they have entered the fluid and substances around him.

1783. The statements also have reference to the fish when in a straight form; if it assume a bent shape, then the lines of force around it vary in their intensity in a manner that may be anticipated theoretically. Thus if the hands were applied at 1, 7, a feebler shock in the arms would be expected if the animal were curved with that side inwards, than if it were straight, because the distance between the parts would be diminished, and the intervening water therefore conduct more of the force. But with respect to the parts *immersed*, or to animals, as fish *in the water* between 1 and 7, they would be more powerfully, instead of less powerfully, shocked.

1784. It is evident from all the experiments, as well as from simple considerations, that all the water and all the conducting matter around the fish through which a discharge circuit can in any way be completed, is filled at the moment with circulating electric power; and this state might be easily represented generally in a diagram by drawing the lines of inductive action (1231. 1304. 1338.) upon it: in the case of a Gymnotus, surrounded equally in all directions by water, these would resemble generally, in disposition, the magnetic curves of a magnet, having the same straight or curved shape as the animal, i. e. provided he, in such cases, employed, as may be expected, his four electric organs at once.

1785. This Gymnotus can stun and kill fish which are in very various positions to its own body; but on one day when I saw it eat, its action seemed to me to be peculiar. A live fish about five inches in length, caught not half a minute before, was dropped into the tub. The Gymnotus instantly turned round in such a manner as to form a coil inclosing the fish, the latter representing a diameter across it; a shock passed, and there in an instant was the fish struck motionless, as if by lightning, in the midst of the waters, its side floating to the light. The Gymnotus made a turn or two to look for its prey, which having found he bolted, and then went searching about for more. A second smaller fish was given him, which being hurt in the conveyance, showed but little signs of life, and this he swallowed at once, apparently without shocking it. The coiling of the Gymnotus round its prey had, in this case, every appearance of being intentional on its part, to increase the force of the shock, and the action is evidently exceedingly well suited for that purpose (1783.), being in full accordance with the well-known laws of the discharge of currents in masses of conducting matter; and though the fish may not always put this artifice in practice, it is very probable he is aware of its advantage, and may resort to it in cases of need.

1786. Living as this animal does in the midst of such a good conductor as water, the first thoughts are thoughts of surprise that it can sensibly electrify anything, but a little consideration soon makes one conscious of many points of great beauty, illustrating the wisdom of the whole arrangement. Thus the very conducting power which the water has; that which it gives to the moistened skin of the fish or animal to be struck; the extent of surface by which the fish and the water conducting the charge to it are in contact; all conduce to favour and increase the shock upon the doomed animal, and are in the most perfect contrast with the inefficient state of things which would exist if the Gymnotus and the fish were surrounded by air; and at the same time that the power is one of low intensity, so that a dry skin wards it off, though a moist one conducts it (1760.); so is it one of great quantity (1770.), that though the surrounding water does conduct away much, enough to produce a full effect may take its course through the body of the fish that is to be caught for food, or the enemy that is to be conquered.

1787. Another remarkable result of the relation of the Gymnotus and its prey to the medium around them is, that the larger the fish to be killed or stunned, the greater will be the shock to which it is subject, though the Gymnotus may exert only an equal power; for the large fish has passing through its body those currents of electricity, which, in the case of a smaller one, would have been conveyed harmless by the water at its sides.

1788. The Gymnotus appears to be sensible when he has shocked an animal, being made conscious of it, probably, by the *mechanical impulse* he receives, caused by the spasms into which it is thrown. When I touched him with my hands, he gave me shock after shock; but when I touched him with glass rods, or the insulated conductors, he gave one or two shocks, felt by others having their hands in at a distance, but then ceased to exert the influence, as if made aware it had not the desired effect. Again, when he has been touched with the conductors several times, for experiments on the galvanometer or other apparatus, and appears to be languid or indifferent, and not willing to give shocks, yet being touched by the hands, they, by convulsive motion, have informed him that a sensitive thing was present, and he has quickly shown his power and his willingness to astonish the experimenter.

1789. It has been remarked by Geoffroy St. Hilaire, that the electric organs of the Torpedo, Gymnotus, and similar fishes, cannot be considered as essentially connected with those which are of high and direct importance to the life of the animal, but to belong rather to the common teguments; and it has also been found that such Torpedos as have been deprived of the use of their peculiar organs, have continued the functions of life quite as well as those in which they were allowed to remain. These, with other considerations, lead me to look at these parts with a hope that they may upon close investigation prove to be a species of natural apparatus, by means of which we may apply the principles of *action and reaction* in the investigation of the nature of the *nervous influence.*

1790. The anatomical relation of the nervous system to the electric organ; the evident exhaustion of the nervous energy during the production of electricity in that organ; the apparently equivalent production of electricity in proportion to the quantity of nervous force consumed; the constant direction of the current produced, with its relation to what we may believe to be an equally constant direction of the nervous energy thrown into action at the same time; all induce me to believe, that it is not impossible but that, on passing electricity per force through the organ, a reaction back upon the nervous system belonging to it might take place, and that a restoration, to a greater or smaller degree, of that which the animal expends in the act of exciting a current, might perhaps be effected. We have the analogy in relation to heat and magnetism. Seebeck taught us how to commute heat into electricity; and Peltier has more lately given us the strict converse of this, and shown us how to convert the electricity into heat, including both its relation of hot and cold. Oersted showed how we were to convert electric into magnetic forces, and I had the delight of adding the other member of the full relation, by reacting back again and converting magnetic into electric forces. So perhaps in these organs, where nature has provided the apparatus by means of which the animal can exert and convert nervous into electric force, we may be able, possessing in that point of view a power far beyond that of the fish itself, to reconvert the electric into the nervous force.

1791. This may seem to some a very wild notion, as assuming that the nervous power is in some degree analogous to such powers as heat, electricity, and magnetism. I am only assuming it, however, as a reason for making certain experiments, which, according as they give positive or negative results, will regulate further expectation. And with respect to the nature of nervous power, that exertion of it which is conveyed along the nerves to the various organs which they excite into action, is not the direct principle of *life*; and therefore I see no natural reason why we should not be allowed in certain cases to *determine* as well as observe its course. Many philosophers think the power is electricity. Priestley put forth this view in 1774 in a very striking and distinct form, both as regards ordinary animals and those which are electric, like the Torpedo[1]. Dr. Wilson Philip considers that the agent in certain nerves is electricity modified by vital action[2]. Matteucci thinks that the nervous fluid or energy, in the nerves belonging to the electric organ at least, is electricity[1]. MM. Prevost and Dumas are of opinion that electricity moves in the nerves belonging to the muscles; and M. Prevost adduces a beautiful experiment, in which steel was magnetized, in proof of this view; which, if it should be confirmed by further observation and by other philosophers, is of the utmost consequence to the progress of this high branch of knowledge[2]. Now though I am not as yet convinced by the facts that the nervous fluid is only electricity, still I think that the agent in the nervous system may be an inorganic force; and if there be reasons for supposing that magnetism is a higher relation of force than electricity (1664. 1731. 1734.), so it may well be imagined that the nervous power may be of a still more exalted character, and yet within the reach of experiment.

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[1] Priestley on Air, vol. i. p. 277. Edition of 1774.

[2] Dr. Wilson Philip is of opinion, that the nerves which excite the muscles and effect the chemical changes of the vital functions, operate by the electric power supplied by the brain and spinal marrow, in its effects, modified by the vital powers of the living animal; because he found, as he informs me, as early as 1815, that while the vital powers remain, all these functions can be as well performed by voltaic electricity after the removal of the nervous influence, as by that influence itself; and in the end of that year he presented a paper to the Royal Society, which was read at one of their meetings, giving an account of the experiments on which this position was founded.

1792. The kind of experiment I am bold enough to suggest is as follows. If a Gymnotus or Torpedo has been fatigued by frequent exertion of the electric organs, would the sending of currents of similar force to those he emits, or of other degrees of force, either continuously or intermittingly in the same direction as those he sends forth, restore him his powers and strength more rapidly than if he were left to his natural repose?

1793. Would sending currents through in the contrary direction exhaust the animal rapidly? There is, I think, reason to believe the Torpedo (and perhaps the Gymnotus) is not much disturbed or excited by electric currents sent only through the electric organ; so that these experiments do not appear very difficult to make.

1794. The disposition of the organs in the Torpedo suggest still further experiments on the same principle. Thus when a current is sent in the natural direction, i. e. from below upwards through the organ on one side of the fish, will it excite the organ on the other side into action? or if sent through in the contrary direction, will it produce the same or any effect on that organ? Will it do so if the nerves proceeding to the organ or organs be tied? and will it do so after the animal has been so far exhausted by previous shocks as to be unable to throw the organ into action in any, or in a similar, degree of his own will?

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[1] Bibliothèque Universelle, 1837, tom. xii. 192.

[2] Ibid, 1837, xii. 202; xiv. 200.

1795. Such are some of the experiments which the conformation and relation of the electric organs of these fishes suggest, as being rational in their performance, and promising in anticipation. Others may not think of them as I do; but I can only say for myself, that were the means in my power, they are the very first that I would make.

Royal Institution,
November 9th, 1838.

SIXTEENTH SERIES.

§ 24. *On the source of power in the voltaic pile.* ¶ i. *Exciting electrolytes, &c. being conductors of thermo and feeble currents.* ¶ ii. *Inactive conducting circles containing an electrolytic fluid.* ¶ iii. *Active circles excited by solution of sulphuret of potassium, &c.*

Received January 28,—Read February 6, 1840.

§ 24. *On the source of power in the voltaic pile.*

1796. WHAT is the source of power in a voltaic pile? This question is at present of the utmost importance in the theory and to the development of electrical science. The opinions held respecting it are various; but by far the most important are the two which respectively find the source of power in contact, and in chemical force. The question between them touches the first principles of electrical action; for the opinions are in such contrast, that two men respectively adopting them are thenceforward constrained to differ, in every point, respecting the probable and intimate nature of the agent or force on which all the phenomena of the voltaic pile depend.

1797. The theory of contact is the theory of Volta, the great discoverer of the voltaic pile itself, and it has been sustained since his day by a host of philosophers, amongst whom, in recent times, rank such men as Pfaff, Marianini, Fechner, Zamboni, Matteucci, Karsten, Bouchardat, and as to the excitement of the power, even Davy; all bright stars in the exalted regions of science. The theory of chemical action was first advanced by Fabroni[1], Wollaston[2], and Parrot[3], and has been more or less developed since by Œrsted, Becquerel, De la Rive, Ritchie, Pouillet, Schœnbein, and many others, amongst whom Becquerel ought to be distinguished as having contributed, from the first, a continually increasing mass of the strongest experimental evidence in proof that chemical action always evolves electricity[1]; and De la Rive should be named as most clear and constant in his views, and most zealous in his production of facts and arguments, from the year 1827 to the present time[2].

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[1] A.D. 1792, 1799. Becquerel's Traité de l'E'lectricité, i. pp. 81—91, and Nicholson's Quarto Journal, iii. 308. iv. 120, or Journal de Physique, vi. 348.

[2] A.D. 1801. Philosophical Transactions, 1801, p. 427.

[3] A.D. 1801. Annales de Chimie, 1829, xlii. 45; 1831, xlvi. 361.

1798. Examining this question by the results of definite electro-chemical action, I felt constrained to take part with those who believed the origin of voltaic power to consist in chemical action alone (875. 965.), and ventured a paper on it in April 1834[3] (875, &c.), which obtained the especial notice of Marianini[4]. The rank of this philosopher, the observation of Fechner[5], and the consciousness that over the greater part of Italy and Germany the contact theory still prevailed, have induced me to re-examine the question most carefully. I wished not merely to escape from error, but was anxious to convince myself of the truth of the contact theory; for it was evident that if contact electromotive force had any existence, it must be a power not merely unlike every other natural power as to the phenomena it could produce, but also in the far higher points of limitation, definite force, and finite production (2065.).

1799. I venture to hope that the experimental results and arguments which have been thus gathered may be useful to science. I fear the detail will be tedious, but that is a necessary consequence of the state of the subject. The contact theory has long had possession of men's minds, is sustained by a great weight of authority, and for years had almost undisputed sway in some parts of Europe. If it be an error, it can only be rooted out by a great amount of forcible experimental evidence; a fact sufficiently clear to my mind by the circumstance, that De la Rive's papers have not already convinced the workers upon this subject. Hence the reason why I have thought it needful to add my further testimony to his and that of others, entering into detail and multiplying facts in a proportion far beyond any which would have been required for the proof and promulgation of a new scientific truth (2017.). In so doing I may occasionally be only enlarging, yet then I hope strengthening, what others, and especially De la Rive, have done.

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[1] A.D. 1824, &c. Annales de Chimie, 1824, xxv. 405; 1827, xxxv. 113; 1831, xlvi. 265, 276, 337; xlvii. 113; xlix. 131.

[2] Ibid. 1828, xxxvii. 225; xxxix. 297; 1836, lxii. 147: or Mémoires de Genève, 1829, iv. 285; 1832, vi. 149; 1835, vii.

[3] Philosophical Transactions, 1834, p. 425.

[4] Memorie della Società Italiana in Modena, 1837, xxi. p. 205.

[5] Philosophical Magazine, 1838, xiii. 205; or Poggendorf's Annalen, xlii. p. 481. Fechner refers also to Pfaff's reply to my paper. I never cease to regret that the German is a sealed language to me.

1800. It will tend to clear the question, if the various views of contact are first stated. Volta's theory is, that the simple contact of conducting bodies causes electricity to be developed at the point of contact without any change in nature of the bodies themselves; and that though such conductors as water and aqueous fluids have this property, yet the degree in which they possess it is unworthy of consideration in comparison with the degree to which it rises amongst the metals[1]. The present views of the Italian and German contact philosophers are, I believe, generally the same, except that occasionally more importance is attached to the contact of the imperfect conductors with the metals. Thus Zamboni (in 1837) considers the metallic contact as the most powerful source of electricity, and not that of the metals with the fluids[2]; but Karsten, holding the contact theory, transfers the electromotive force to the contact of the fluids with the solid conductors[3]. Marianini holds the same view of the principle of contact, with this addition, that actual contact is not required to the exertion of the exciting force, but that the two approximated dissimilar conductors may affect each other's state, when separated by sensible intervals of the $\frac{1}{10000}$dth of a line and more, air intervening[4].

1801. De la Rive, on the contrary, contends for simple and strict chemical action, and, as far as I am aware, admits of no current in the voltaic pile that is not conjoined with and dependent upon a complete chemical effect. That admirable electrician Becquerel, though expressing himself with great caution, seems to admit the possibility of chemical attractions being able to produce electrical currents when they are not strong enough to overcome the force of cohesion, and so terminate in combination[1]. Schœnbein states that a current may be produced by a tendency to chemical action, i. e. that substances which have a tendency to unite chemically may produce a current, though that tendency is not followed up by the actual combination of the substances[2]. In these cases the assigned force becomes the same as the contact of Volta, inasmuch as the acting matters are not altered whilst producing the current. Davy's opinion was, that contact like that of Volta excited the current or was the cause of it, but that chemical changes supplied the current. For myself I am at present of the opinion which De la Rive holds, and do not think that, in the voltaic pile, mere contact does anything in the excitation of the current, except as it is preparatory to, and ends in, complete chemical action (1741. 1745.).

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[1] Annales de Chimie, 1802, xl. p. 225.

[2] Bibliothèque Universelle, 1836, v. 387; 1837, viii. 189.

[3] L'Institut, No. 150.

[4] Mem. della Soc. Ital. in Modena, 1837, xxi. 232—237.

1802. Thus the views of contact vary, and it may be said that they pass gradually from one to another, even to the extent of including chemical action: but the two extremes appear to me irreconcilable in principle under any shape; they are as follows. The contact theory assumes, that when two different bodies being conductors of electricity are in contact, there is a force at the point of contact by which one of the bodies gives a part of its natural portion of electricty to the other body, which the latter takes in addition to its own natural portion; that, though the touching points have thus respectively given and taken electricity, they cannot retain the charge which their contact has caused, but discharge their electricities to the masses respectively behind them (2067.): that the force which, at the point of contact, induces the particles to assume a new state, cannot enable them to keep that state (2069.): that all this happens without any permanent alteration of the parts that are in contact, and has no reference to their chemical forces (2065. 2069.).

1803. The chemical theory assumes, that at the place of action, the particles which are in contact act chemically upon each other and are able, under the circumstances, to throw more or less of the acting force into a dynamic form (947. 996. 1120.): that in the most favourable circumstances, the whole is converted into dynamic force (1000.): that then the amount of current force produced is an exact equivalent of the original chemical force employed; and that in no case (in the voltaic pile) can any electric current be produced, without the active exertion and consumption of an equal amount of chemical force, ending in a given amount of chemical change.

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[1] Annales de Chimie, 1835, lx. 171; and Traité de l'E'lectricité, i. pp. 253, 258.

[2] Philosophical Magazine, 1838, xii. 227, 311, 314; also Bibliothèque Universelle, 1838, xiv. 155, 395.

1804. Marianini's paper[1] was to me a great motive for reexamining the subject; but the course I have taken was not so much for the purpose of answering particular objections, as for the procuring evidence, whether relating to controverted points or not, which should be satisfactory to my own mind, open to receive either one theory or the other. This paper, therefore, is not controversial, but contains further facts and proofs of the truth of De la Rive's views. The cases Marianini puts are of extreme interest, and all his objections must, one day, be answered, when numerical results, both as to intensity and quantity of force, are obtained; but they are all debateable, and, to my mind, depend upon variations of quantity which do not affect seriously the general question. Thus, when that philosopher quotes the numerical results obtained by considering two metals with fluids at their opposite extremities which tend to form counter currents, the difference which he puts down to the effect of metallic contact, either made or interrupted, I think accountable for, on the facts partly known respecting opposed currents; and with me differences quite as great, and greater, have arisen, and are given in former papers (1046.), when metallic contacts were in the circuit. So at page 213 of his memoir, I cannot admit that *e* should give an effect equal to the difference of *b* and *d*; for in *b* and *d* the opposition presented to the excited currents is merely that of a bad conductor, but in the case of *e* the opposition arises from the power of an opposed acting source of a current.

1805. As to the part of his memoir respecting the action of sulphuretted solutions[1], I hope to be allowed to refer to the investigations made further on. I do not find, as the Italian philosopher, that iron with gold or platina, in solution of the sulphuret of potassa, is positive to them[2], but, on the contrary, powerfully negative, and for reasons given in the sequel (2049.).

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[1] Memorie della Società Italiana in Modena, 1827, xxi. p. 205.

1806. With respect to the discussion of the cause of the spark before contact[3], Marianini admits the spark, but I give it up altogether. Jacobi's paper[4] convinces me I was in error as to *that proof* of the existence of a state of tension in the metals before contact (915. 956.). I need not therefore do more at present than withdraw my own observations.

1807. I now proceed to address myself to the general argument, rather than to particular controversy, or to the discussion of cases feeble in power and doubtful in nature; for I have been impressed from the first with the feeling that it is no weak influence or feeble phenomenon that we have to account for, but such as indicates a force of extreme power, requiring, therefore, that the cause assigned should bear some proportion, both in intensity and quantity, to the effects produced.

1808. The investigations have all been made by aid of currents and the galvanometer, for it seemed that such an instrument and such a course were best suited to an examination of the electricity of the voltaic pile. The electrometer is no doubt a most important instrument, but the philosophers who do use it are not of accord in respect to the safety and delicacy of its results. And even if the few indications as yet given by the electrometer be accepted as correct, they are far too general to settle the question of, whether contact or chemical action is the exciting force in the voltaic battery. To apply that instrument closely and render it of any force in supplying affirmative arguments to either theory, it would be necessary to construct a table of contacts, or the effects of contacts, of the different metals and fluids concerned in the construction of the voltaic pile, taken in pairs (1868.), expressing in such table both the *direction* and the *amount* of the contact force.

1809. It is assumed by the supporters of the contact theory, that though the metals exert strong electromotive forces at their points of contact with each other, yet these are so balanced in a metallic circuit that no current is ever produced whatever their arrangement may be. So in Plate III. fig. 1. if the contact force of copper and zinc is 10 ⟶, and a third metal be introduced at *m*, the effect of its contacts, whatever that metal may be, with the zinc and copper at *b* and *c*, will be an amount of force in the opposite direction = 10. Thus, if it were potassium, its contact force at *b* might be 5 ⟶, but then its contact force at *c* would be ⟵ 15: or if it were gold, its contact force at *b* might be ⟵ 19, but then its contact force at *c* would be 9 ⟶. This is a very large assumption, and that the theory may agree with the facts is necessary: still it is, I believe, only an assumption, for I am not aware of any data, independent of the theory in question, which prove its truth.

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[1] Memorie della Società Italiana in Modena, 1827, xxi. p. 217.

[2] Ibid. p. 217.

[3] Ibid. p. 225.

[4] Philosophical Magazine, 1838, xiii. 401.

1810. On the other hand, it is assumed that fluid conductors, and such bodies as contain water, or, in a word, those which I have called electrolytes (664. 823. 921.), either exert no contact force at their place of contact with the metals, or if they do exert such a power, then it is with this most important difference, that the forces are not subject to the same law of compensation or neutralization in the complete circuit, as holds with the metals (1809.). But this, I think I am justified in saying, is an assumption also, for it is supported not by any independent measurement or facts (1808.), but only by the theory which it is itself intended to support.

1811. Guided by this opinion, and with a view to ascertain what is, in an active circle, effected by contact and what by chemical action, I endeavoured to find some bodies in this latter class (1810.) which should be without chemical action on the metals employed, so as to exclude that cause of a current, and yet such good conductors of electricity as to show any currents due to the contact of these metals with each other or with the fluid: concluding that any electrolyte which would conduct the thermo current of a single pair of bismuth and antimony plates would serve the required purpose, I sought for such, and fortunately soon found them.

¶ i. *Exciting electrolytes, &c., being conductors of thermo and feeble currents.*

1812. *Sulphuret of potassium.*—This substance and its solution were prepared as follows. Equal weights of caustic potash (potassa fusa) and sulphur were mixed with and heated gradually in a Florence flask, till the whole had fused and united, and the sulphur in excess began to sublime. It was then cooled and dissolved in water, so as to form a strong solution, which by standing became quite clear.

1813. A portion of this solution was included in a circuit containing a galvanometer and a pair of antimony and bismuth plates; the connexion with the electrolyte was made by two platinum plates, each about two inches long and half an inch wide: nearly the whole of each was immersed, and they were about half an inch apart. When the circuit was completed, and all at the same temperature, there was no current; but the moment the junction of the antimony and bismuth was either heated or cooled, the corresponding thermo current was produced, causing the galvanometer-needle to be permanently deflected, occasionally as much as 80°. Even the small difference of temperature occasioned by touching the Seebeck element with the finger, produced a very sensible current through the electrolyte. When in place of the antimony-bismuth combination mere wires of *copper and platinum*, or *iron and platinum* were used, the application of the spirit-lamp to the junction of these metals produced a thermo current which instantly travelled round the circuit.

1814. Thus this electrolyte will, as to high conducting power, fully answer the condition required (1811.). It is so excellent in this respect, that I was able to send the thermo current of a single Seebeck's element across five successive portions connected with each other by platinum plates.

1815. *Nitrous acid.*—Yellow anhydrous nitrous acid, made by distilling dry nitrate of lead, being put into a glass tube and included in a circuit with the antimony-bismuth arrangement and the galvanometer, gave no indication of the passage of the thermo current, though the immersed electrodes consisted each of about four inches in length of moderately thick platinum wire, and were not above a quarter of an inch apart.

1816. A portion of this acid was mixed with nearly its volume of pure water; the resulting action caused depression of temperature, the evolution of some nitrous gas, the formation of some nitric acid, and a dark green fluid was produced. This was now such an excellent conductor of electricity, that almost the feeblest current could pass it. That produced by Seebeck's circle was sensible when only one-eighth of an inch in length of the platinum wires dipped in the acid. When a couple of inches of each electrode was in the fluid, the conduction was so good, that it made very little difference at the galvanometer whether the platinum wires touched each other in the fluid, or were a quarter of an inch apart[1].

1817. *Nitric acid.*—Some pure nitric acid was boiled to drive off all the nitrous acid, and then cooled. Being included in the circuit by platinum plates (1813.), it was found to conduct so badly that the effect of the antimony-bismuth pair, when the difference of temperature was at the greatest, was scarcely perceptible at the galvanometer.

1818. On using a pale yellow acid, otherwise pure, it was found to possess rather more conducting power than the former. On employing a red nitric acid, it was found to conduct the thermo current very well. On adding some of the green nitrous acid (1816.) to the colourless nitric acid, the mixture acquired high conducting powers. Hence it is evident that nitric acid is not a good conductor when pure, but that the presence of nitrous acid in it (conjointly probably with water), gives it this power in a very high degree amongst electrolytes[2]. A very red strong nitric acid, and a weak green acid, (consisting of one vol. strong nitric acid and two vols. of water, which had been rendered green by the action of the negative platinum electrode of a voltaic battery,) were both such excellent conductors that the thermo current could pass across five separate portions of them connected by platinum plates, with so little retardation, that I believe twenty interruptions would not have stopped this feeble current.

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[1] De la Rive has pointed out the facility with which an electric current passes between platinum and nitrous acid. Annales de Chimie, 1828, xxxvii. 278.

[2] Schœnbein's experiments on a compound of nitric and nitrous acids will probably bear upon and illustrate this subject. Bibliothèque Universelle, 1817, x. 406.

1819. *Sulphuric acid.*—Strong oil of vitriol, when between platinum electrodes (1813.), conducted the antimony-bismuth thermo current sensibly, but feebly. A mixture of two volumes acid and one volume water conducted much better, but not nearly so well as the two former electrolytes (1814. 1816.). A mixture of one volume of oil of vitriol and two volumes saturated solution of sulphate of copper conducted this feeble current very fairly.

Potassa.—A strong solution of caustic potassa, between platinum plates, conducted the thermo current sensibly, but very feebly.

1820. I will take the liberty of describing here, as the most convenient place, other results relating to the conducting power of bodies, which will be required hereafter in these investigations. Galena, yellow sulphuret of iron, arsenical pyrites, native sulphuret of copper and iron, native gray artificial sulphuret of copper, sulphurets of bismuth, iron, and copper, globules of oxide of burnt iron, oxide of iron by heat or scale oxide, conducted the thermo current very well. Native peroxide of manganese and peroxide of lead conducted it moderately well.

1821. The following are bodies, in some respect analogous in nature and composition, which did not sensibly conduct this weak current when the contact surfaces were small:—artificial gray sulphuret of tin, blende, cinnabar, hæmatite, Elba iron-ore, native magnetic oxide of iron, native peroxide of tin or tinstone, wolfram, fused and cooled protoxide of copper, peroxide of mercury.

1822. Some of the foregoing substances are very remarkable in their conducting power. This is the case with the solution of sulphuret of potassium (1813.) and the nitrous acid (1816.), for the great amount of this power. The peroxide of manganese and lead are still more remarkable for possessing this power, because the *protoxides* of these metals do not conduct either the feeble thermo current or a far more powerful one from a voltaic battery. This circumstance made me especially anxious to verify the point with the peroxide of lead. I therefore prepared some from red-lead by the action of successive portions of nitric acid, then boiled the brown oxide, so obtained, in several portions of distilled water, for days together, until every trace of nitric acid and nitrate of lead had been removed; after which it was well and perfectly dried. Still, when a heap of it in powder, and consequently in very imperfect contact throughout its own mass, was pressed between two plates of platinum and so brought into the thermo-electric circuit (1813.), the current was found to pass readily.

¶ ii. *Inactive conducting circles containing a fluid or electrolyte.*

1823. De la Rive has already quoted the case of potash, iron and platina[1], to show that where there was no chemical action there was no current. My object is to increase the number of such cases; to use other fluids than potash, and such as have good conducting power for weak currents; to use also strong and weak solutions; and thus to accumulate the conjoint experimental and argumentative evidence by which the great question must finally be decided.

1824. I first used the sulphuret of potassium as an electrolyte of good conducting power, but chemically inactive (1811.) when associated with iron and platinum in a circuit. The arrangement is given in fig. 2, where D, E represent two test-glasses containing the strong solution of sulphuret of potassium (1812.); and also four metallic plates, about 0·5 of an inch wide and two inches long in the immersed part, of which the three marked P, P, P were platinum, and that marked I, of clean iron: these were connected by iron and platinum wires, as in fig. 2, a galvanometer being introduced at G. In this arrangement there were three metallic contacts of platinum and iron, *a b* and *x*: the first two, being opposed to each other, may be considered as neutralizing each other's forces; but the third, being unopposed by any other metallic contact, can be compared with either the difference of *a* and *b* when one is warmer than the other, or with itself when in a heated or cooled state (1830.), or with the force of chemical action when any body capable of such action is introduced there (1831.).

1825. When this arrangement is completed and in order, there is absolutely no current circulating through it, and the galvanometer-needle rests at 0°; yet is the whole circuit open to a very feeble current, for a difference of temperature at any one of the junctions *a*, *b*, or *x*, causes a corresponding thermo current, which is instantly detected by the galvanometer, the needle standing permanently at 30° or 40°, or even 50°.

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[1] Philosophical Magazine, 1837, xi. 275.

1826. But to obtain this proper and normal state, it is necessary that certain precautions be attended to. In the first place, if the circuit be complete in every part except for the immersion of the iron and platinum plates into the cup D, then, upon their introduction, a current will be produced directed from the platinum (which appears to be positive) through the solution to the iron; this will continue perhaps five or ten minutes, or if the iron has been carelessly cleaned, for several hours; it is due to an action of the sulphuretted solution on *oxide of iron*, and not to any effect on the metallic iron; and when it has ceased, the disturbing cause may be considered as exhausted. The experimental proofs of the truth of this explanation, I will quote hereafter (2049.).

1827. Another precaution relates to the effect of accidental movements of the plates in the solution. If two platinum plates be put into a solution of this sulphuret of potassium, and the circuit be then completed, including a galvanometer, the arrangement, if perfect, will show no current; but if one of the plates be lifted up into the air for a few seconds and then replaced, it will be negative to the other, and produce a current lasting for a short time[1]. If the two plates be iron and platinum, or of any other metal or substance not acted on by the sulphuret, the same effect will be produced. In these cases, the current is due to the change wrought by the air on the film of sulphuretted solution adhering to the removed plate[2]; but a far less cause than this will produce a current, for if one of the platinum plates be removed, washed well, dried, and even heated, it will, on its re-introduction, almost certainly exhibit the negative state for a second or two.

1828. These or other disturbing causes appear the greater in these experiments in consequence of the excellent conducting power of the solution used; but they do not occur if care be taken to avoid any disturbance of the plates or the solution, and then, as before said, the whole acquires a normal and perfectly inactive state.

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[1] Marianini observed effects of this kind produced by exposure to the air, of one of two plates dipped in nitric acid. Annales de Chimie, 1830, xlv. p. 42.

[2] Becquerel long since referred to the effect of such exposure of a plate, dipped in certain solutions, to the air. Generally the plate so exposed became positive on re-immersion. Annales de Chimie, 1824, xxv. 405.

1829. Here then is an arrangement in which the contact of platinum and iron at *x* is at liberty to produce any effect which such a contact may have the power of producing; and yet what is the consequence? absolutely nothing. This is not because the electrolyte is so bad a conductor that a current of contact cannot pass, for currents far feebler than this is assumed to be, pass readily (1813.); and the electrolyte employed is vastly superior in conducting power to those which are commonly used in voltaic batteries or circles, in which the current is still assumed to be dependent upon contact. The simple conclusion to which the experiment should lead is, in my opinion, that the contact of iron and platinum is absolutely without any electromotive force (1835. 1859. 1889.).

1830. If the contact be made really active and effective, according to the beautiful discovery of Seebeck, by making its temperature different to that of the other parts of the circuit, then its power of generating a current is shown (1824.). This enables us to compare the supposed power of the mere contact with that of a thermo contact; and we find that the latter comes out as infinitely greater than the former, for the former is nothing. The same comparison of mere contact and thermo contact may be made by contrasting the effect of the contact *c* at common temperatures, with either the contact at *a* or at *b*, either heated or cooled. Very moderate changes of temperature at these places produce instantly the corresponding current, but the mere contact at *x* does nothing.

1831. So also I believe that a true and philosophic and even rigid comparison may be made at *x*, between the assumed effect of mere contact and that of chemical action. For if the metals at *x* be separated, and a piece of paper moistened in dilute acid, or a solution of salt, or if only the tongue or a wet finger be applied there, then a current is caused, stronger by far than the thermo currents before produced (1830.), passing from the iron through the introduced acid or other active fluid to the platinum. This is a case of current from chemical action without any metallic contact in the circuit on which the effect can for a moment be supposed to depend (879.); it is even a case where metallic contact is changed for chemical action, with the result, that where contact is found to be quite ineffectual, chemical action is very energetic in producing a current.

1832. It is of course quite unnecessary to say that the same experimental comparisons may be made at either of the other contacts, *a* or *b*.

1833. Admitting for the moment that the arrangement proves that the contact of platinum and iron at *x* has no electromotive force (1835. 1859.), then it follows also that the contact of either platinum or iron with any other metal has no such force. For if another metal, as zinc, be interposed between the iron and platinum at *x*, fig 2, no current is produced; and yet the test application of a little heat at *a* or *b*, will show by the corresponding current, that the circuit being complete will conduct any current that may tend to pass. Now that the contacts of zinc with iron and with platinum are of equal electromotive force, is not for a moment admitted by those who support the theory of contact activity; we ought therefore to have a resulting action equal to the differences of the two forces, producing a certain current. No such current is produced, and I conceive, with the admission above, that such a result proves that the contacts *iron-zinc* and *platinum-zinc* are entirely without electromotive force.

1834. Gold, silver, potassium, and copper were introduced at *x* with the like negative effect; and so no doubt might every other metal, even according to the relation admitted amongst the metals by the supporters of the contact theory (1809.). The same negative result followed upon the introduction of many other conducting bodies at the same place; as, for instance, those already mentioned as easily conducting the thermo current (1820.); and the effect proves, I think, that the contact of any of these with either iron or platinum is utterly ineffective as a source of electromotive force.

1835. The only answer which, as it appears to me, the contact theory can set up in opposition to the foregoing facts and conclusions is, to say that the solution of sulphuret of potassium in the cup D, fig. 2, acts as a metal would do (1809.), and so the effects of all the contacts in the circuit are exactly balanced.

I will not stop at this moment to show that the departure with respect to electrolytes, or the fluid bodies in the voltaic pile, *from the law* which is supposed to hold good with the metals and solid conductors, though only an assumption, is still essential to the contact theory of the voltaic pile (1810. 1861.)[1]; nor to prove that the electrolyte is no otherwise like the metals than in having no contact electromotive force whatever. But believing that this will be very evident shortly, I will go on with the experimental results, and resume these points hereafter (1859. 1889.).

1836. The experiment was now repeated with the substitution of a bar of *nickel* for that of iron, fig. 2 (1824.), all other things remaining the same[2]. The circuit was again found to be a good conductor of a feeble thermo current, but utterly inefficient as a voltaic circuit when all was at the same temperature, and due precautions taken (2051.). The introduction of metals at the contact x was as ineffective as before (1834.); the introduction of chemical action at x was as striking in its influence as in the former case (1831.); all the results were, in fact, parallel to those already obtained; and if the reasoning then urged was good, it will now follow that the contact of platinum and nickel with each other, or of either with any of the different metals or solid conductors introduced at x, is entirely without electromotive force[3].

1837. Many other pairs of metals were compared together in the same manner; the solution of sulphuret of potassium connecting them together at one place, and their mutual contact doing that office at another. The following are cases of this kind: iron and gold; iron and palladium; nickel and gold; nickel and palladium; platina and gold; platina and palladium. In all these cases the results were the same as those already given with the combinations of platinum and iron.

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[1] See Fechner's words. Philosophical Magazine, 1838, xiii. 377.

[2] There is another form of this experiment which I sometimes adopted, in which the cup E, fig. 2, with its contents, was dismissed, and the platinum plates in it connected together. The arrangement may then be considered as presenting three contacts of iron and platinum, two acting in one direction, and one in the other. The arrangement and the results are virtually the same as those already given. A still simpler but equally conclusive arrangement for many of the arguments, is to dismiss the iron between a and b altogether, and so have but one contact, that at x, to consider.

[3] One specimen of nickel was, on its immersion, positive to platinum for seven or eight minutes, and then became neutral. On taking it out it seemed to have a yellowish tint on it, as if invested by a coat of sulphuret; and I suspected this piece had acted like lead (1885.) and bismuth (1895.). It is difficult to get pure and also perfectly compact nickel; and if porous, then the matter retained in the pores produces currents.

1838. It is necessary that due precaution be taken to have the arrangements in an unexceptionable state. It often happened that the first immersion of the plates gave deflections; it is, in fact, almost impossible to put two plates of the *same metal* into the solution without causing a deflection; but this generally goes off very quickly, and then the arrangement may be used for the investigation (1826.). Sometimes there is a feeble but rather permanent deflection of the needle; thus when platinum and palladium were the metals, the first effect fell and left a current able to deflect the galvanometer-needle 3°, indicating the platinum to be positive to the palladium. This effect of 3°, however, is almost nothing compared to what a mere thermo current can cause, the latter producing a deflection of 60° or more; besides which, even supposing it an essential effect of the arrangement, it is in the wrong direction for the contact theory. I rather incline to refer it to that power which platinum and other substances have of effecting combination and decomposition without themselves entering into union; and I have occasionally found that when a platinum plate has been left for some hours in a strong solution of sulphuret of potassium (1812.) a small quantity of sulphur has been deposited upon it. Whatever the cause of the final feeble current may be, the effect is too small to be of any service in support of the contact theory; while, on the other hand, it affords delicate, and, therefore, strong indications in favour of the chemical theory.

1839. A change was made in the form and arrangement of the cup D, fig. 2, so as to allow of experiments with other bodies than the metals. The solution of sulphuret of potassium was placed in a shallow vessel, the platinum plate was bent so that the immersed extremity corresponded to the bottom of the vessel; on this a piece of loosely folded cloth was laid in the solution, and on that again the mineral or other substance to be compared with the platinum; the fluid being of such depth that only part of that substance was in it, the rest being clean and dry; on this portion the platinum wire, which completed the circuit, rested. The arrangement of this part of the circuit is given in section at fig. 3, where H represents a piece of galena to be compared with the platinum P.

1840. In this way galena, compact yellow copper pyrites, yellow iron pyrites, and globules of oxide of burnt iron, were compared with platinum, (the solution of sulphuret of potassium being the electrolyte used in the circuit,) and with the same results as were before obtained with metals (1829. 1833.).

1841. Experiments hereafter to be described gave arrangements in which, with the same electrolyte, sulphuret of lead was compared with gold, palladium, iron, nickel, and bismuth (1885. 1886.); also sulphuret of bismuth with platinum, gold, palladium, iron, nickel, lead, and sulphuret of lead (1894.), and always with the same result. Where no chemical action occurred there no current was formed; although the circuit remained an excellent conductor, and the contact existed by which, it is assumed in the contact theory, such a current should be produced.

1842. Instead of the strong solution, a dilute solution of the yellow sulphuret of potassium, consisting of one volume of strong solution (1812.) and ten volumes of water, was used. Plates of platinum and iron were arranged in this fluid as before (1824.): at first the iron was negative (2049.), but in ten minutes it was neutral, and the needle at 0°[1]. Then a weak chemical current excited at x (1831.) easily passed: and even a thermo current (1830.) was able to show its effects at the needle. Thus a strong or a weak solution of this electrolyte showed the same phenomena. By diluting the solution still further, a fluid could be obtained in which the iron was, after the first effect, permanently but feebly positive. On allowing time, however, it was found that in all such cases black sulphuret formed here and there on the iron. Rusted iron was negative to platinum (2049.) in this very weak solution, which by direct chemical action could render metallic iron positive.

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[1] Care was taken in these and the former similar cases to discharge the platinum surface of any reacting force it might acquire from the action of the previous current, by separating it from the other metals, and touching it in the liquid for an instant with another platinum plate.

1843. In all the preceding experiments the electrolyte used has been the sulphuret of potassium solution; but I now changed this for another, very different in its nature, namely, the *green nitrous acid* (1816.), which has already been shown to be an excellent conductor of electricity. Iron and platinum were the metals employed, both being in the form of wires. The vessel in which they were immersed was a tube like that formerly described (1815.); in other respects the arrangement was the same in principle as those already used (1824. 1836.). The first effect was the production of a current, the iron being positive in the acid to the platina; but this *quickly ceased,* and the galvanometer-needle came to 0°. In this state, however, the circuit could not in all things be compared with the one having the solution of sulphuret of potassium for its electrolyte (1824.); for although it could conduct the thermo current of antimony and bismuth in a certain degree, yet that degree was very small compared to the power possessed by the former arrangement, or to that of a circle in which the nitrous acid was between two platinum plates (1816.). This remarkable retardation is consequent upon the assumption by the iron of that peculiar state which Schœnbein has so well described and illustrated by his numerous experiments and investigations. But though it must be admitted that the iron in contact with the acid is in a peculiar state (1951. 2001. 2033.), yet it is also evident that a circuit consisting of platinum, iron, peculiar iron, and nitrous acid, does not cause a current though it have sufficient conducting power to carry a thermo current.

1844. But if the contact of platinum and iron has an electromotive force, why does it not produce a current? The application of heat (1830.), or of a little chemical action (1831.) at the place of contact, does produce a current, and in the latter case a strong one. Or if any other of the contacts in the arrangement can produce a current, why is not that shown by some corresponding effect? The only answers are, to say, that the peculiar iron has the same electromotive properties and relations as platinum, or that the nitrous acid is included under the same law with the metals (1809. 1835.); and so the sum of the effects of all the contacts in the circuit is nought, or an exact balance of forces. That the iron is like the platinum in having no electromotive force at its contacts without chemical action, I believe; but that it is unlike it in its electrical relations, is evident from the difference between the two in strong nitric acid, as well as in weak acid; from their difference in the power of transmitting electric currents to either nitric acid or sulphuret of potassium, which is very great; and also by other differences. That the nitrous acid is, as to the power of its contacts, to be separated from other electrolytes and classed with the metals in what is, with them, only an assumption, is a gratuitous mode of explaining the difficulty, which will come into consideration, with the case of sulphuret of potassium, hereafter (1835. 1859. 1889. 2060.).

1845. To the electro-chemical philosopher, the case is only another of the many strong instances, showing that where chemical action is absent in the voltaic circuit, there no current can be formed; and that whether solution of sulphuret of potassium or nitrous acid be the electrolyte or connecting fluid used, still the results are the same, and contact is shown to be inefficacious as an active electromotive condition.

1846. I need not say that the introduction of different metals between the iron and platinum at their point of contact, produced no difference in the results (1833. 1834.) and caused no current; and I have said that heat and chemical action applied there produced their corresponding effects. But these parallels in action and non-action show the identity in nature of this circuit, (notwithstanding the production of the surface of peculiar iron on that metal,) and that with solution of sulphuret of potassium: so that all the conclusions drawn from it apply here; and if that case ultimately stand firm as a proof against the theory of contact force, this will stand also.

1847. I now used oxide of iron and platinum as the extremes of the solid part of the circuit, and the nitrous acid as the fluid; i. e. I heated the iron wire in the flame of a spirit-lamp, covering it with a coat of oxide in the manner recommended by Schœnbein in his investigations, and then used it instead of the clean iron (1843.). The oxide of iron was at first in the least degree positive, and then immediately neutral. This circuit, then, like the former, gave no current at common temperatures; but it differed much from it in conducting power, being a very excellent conductor of a thermo current, the oxide of iron not offering that obstruction to the passage of the current which the peculiar iron did (1843. 1844.). Hence scale oxide of iron and platinum produce no current by contact, the third substance in the proof circuit being nitrous acid; and so the result agrees with that obtained in the former case, where that third substance was solution of sulphuret of potassium.

1848. In using nitrous acid it is necessary that certain precautions be taken, founded on the following effect. If a circuit be made with the green nitrous acid, platinum wires, and a galvanometer, in a few seconds all traces of a current due to first disturbances will disappear; but if one wire be raised into the air and instantly returned to its first position, a current is formed, and that wire is negative, across the electrolyte, to the other. If one wire be dipped only a small distance into the acid, as for instance one fourth of an inch, then the raising that wire not more than one eighth of an inch and instantly restoring it, will produce the same effect as before. The effect is due to the evaporation of the nitrous acid from the exposed wire (1937.). I may perhaps return to it hereafter, but wish at present only to give notice of the precaution that is required in consequence, namely, to retain the immersed wires undisturbed during the experiment.

1849. Proceeding on the facts made known by Schœnbein respecting the relation of iron and nitric acid, I used that acid as the fluid in a voltaic circuit formed with iron and platinum. Pure nitric acid is so deficient in conducting power (1817.) that it may be supposed capable of stopping any current due to the effect of contact between the platinum and iron; and it is further objectionable in these experiments, because, acting feebly on the iron, it produces a chemically excited current, which may be considered as mingling its effect with that of contact: whereas the object at present is, by excluding such chemical action, to lay bare the influence of contact alone. Still the results with it are consistent with the more perfect ones already described; for in a circuit of iron, platinum, and nitric acid, the joint effects of the chemical action on the iron and the contact of iron and platinum, being to produce a current of a certain constant force indicated by the galvanometer, a little chemical action, brought into play where the iron and platinum were in contact as before (1831.), produced a current far stronger than that previously existing. If then, from the weaker current, the part of the effect due to chemical action be abstracted, how little room is there to suppose that any effect is due to the contact of the metals!

1850. But a *red nitric acid* with platinum plates conducts a thermo current well, and will do so even when considerably diluted (1818.). When such red acid is used between iron and platinum, the conducting power is such, that one half of the permanent current can be overcome by a counter thermo current of bismuth and antimony. Thus a sort of comparison is established between a thermo current on the one hand, and a current due to the joint effects of chemical action on iron and contact of iron and platinum on the other. Now considering the admitted weakness of a thermo current, it may be judged what the strength of that part of the second current due to contact can, at the utmost, be; and how little it is able to account for the strong currents produced by ordinary voltaic combinations.

1851. If for a clean iron wire one oxidized in the flame of a spirit-lamp be used, being associated with platinum in pure strong nitric acid, there is a feeble current, the oxide of iron being positive to the platinum, and the facts mainly as with iron. But the further advantage is obtained of comparing the contact of strong and weak acid with this oxidized wire. If one volume of the strong acid and four volumes of water be mixed, this solution may be used, and there is even less deflection than with the strong acid: the iron side is now not sensibly active, except the most delicate means be used to observe the current. Yet in both cases if a chemical action be introduced in place of the contact, the resulting current passes well, and even a thermo current can be made to show itself as more powerful than any due to contact.

1852. In these cases it is safest to put the whole of the oxidized iron under the surface and connect it in the circle by touching it with a platinum wire; for if the oxidized iron be continued through from the acid to the air, it is almost certain to suffer from the joint action of the acid and air at their surface of contact.

1853. I proceeded to use a fluid differing from any of the former: this was solution of *potassa*, which has already been employed by De la Rive (1823.) with iron and platina, and which when strong has been found to be a substance conducting so well, that even a thermo current could pass it (1819.), and therefore fully sufficient to show a contact current, if any such exists.

1854. Yet when a strong solution of this substance was arranged with silver and platinum, (bodies differing sufficiently from each other when connected by nitric or muriatic acid,) as in the former cases, a very feeble current was produced, and the galvanometer-needle stood nearly at zero. The contact of these metals therefore did not appear to produce a sensible current; and, as I fully believe, because no electromotive power exists in such contact. When that contact was exchanged for a very feeble chemical action, namely, that produced by interposing a little piece of paper moistened in dilute nitric acid (1831.), a current was the result. So here, as in the many former cases, the arrangement with a little chemical action and no metallic contact produces a current, but that without the chemical action and with the metallic contact produces none.

1855. Iron or nickel associated with platinum in this strong solution of potassa was positive. The force of the produced current soon fell, and after an hour or so was very small. Then annulling the metallic contact at *x*, fig. 2, and substituting a feeble chemical action there, as of dilute nitric acid, the current established by the latter would pass and show itself. Thus the cases are parallel to those before mentioned (1849, &c.), and show how little contact alone could do, since the effect of the conjoint contact of iron and platinum and chemical action of potash and iron were very small as compared with the contrasted chemical action of the dilute nitric acid.

1856. Instead of a strong solution of potassa, a much weaker one consisting of one volume of strong solution and six volumes of water was used, but the results with the silver and platinum were the same: no current was produced by the metallic contact as long as that only was left for exciting cause, but on substituting a little chemical action in its place (1831.), the current was immediately produced.

1857. Iron and nickel with platinum in the weak solution also produced similar results, except that the positive state of these metals was rather more permanent than with the strong solution. Still it was so small as to be out of all proportion to what was to be expected according to the contact theory.

1858. Thus these different contacts of metals and other well-conducting solid bodies prove utterly inefficient in producing a current, as well when solution of potassa is the third or fluid body in the circuit, as when that third body is either solution of sulphuret of potassium, or hydrated nitrous acid, or nitric acid, or mixed nitric and nitrous acids. Further, all the arguments respecting the inefficacy of the contacts of bodies interposed at the junction of the two principal solid substances, which were advanced in the case of the sulphuret of potassium solution (1833.), apply here with potassa; as they do indeed in every case of a conducting circuit where the interposed fluid is without chemical action and no current is produced. If a case could be brought forward in which the interposed fluid is without action, is yet a sufficiently good conductor, and a current *is* produced; then, indeed, the theory of contact would find evidence in its favour, which, as far as I can perceive, could not be overcome. I have most anxiously sought for such a case, but cannot find one (1798.).

1859. The argument is now in a fit state for the resumption of that important point before adverted to (1835. 1844.), which, if truly advanced by an advocate for the contact theory, would utterly annihilate the force of the previous experimental results, though it would not enable that theory to give a reason for the activity of, and the existence of a current in, the pile; but which, if in error, would leave the contact theory utterly defenceless and without foundation.

1860. A supporter of the contact theory may say that the various conducting electrolytes used in the previous experiments are like the metals; i. e. that they have an electromotive force at their points of contact with the metals and other solid conductors employed to complete the circuit; but that this is of such consistent strength at each place of contact, that, in a complete circle, the sum of the forces is 0 (1809.). The actions at the contacts are tense electromotive actions, but balanced, and so no current is produced. But what experiment is there to support this statement? where are the measured electromotive results proving it (1808.)? I believe there are none.

1861. The contact theory, after assuming that mere contacts of dissimilar substances have electromotive powers, further assumes a difference between metals and liquid conductors (1810.) without which it is impossible that the theory can explain the current in the voltaic pile: for whilst the contact effects in a metallic circuit are assumed to be always perfectly balanced, it is also assumed that the contact effects of the electrolytes or interposed fluid with the metals are not balanced, but are so far removed from anything like an equilibrium, as to produce most powerful currents, even the strongest that a voltaic pile can produce. If so, then why should the solution of sulphuret of potassium be an exception? it is quite unlike the metals: it does not appear to conduct without decomposition; it is an excellent electrolyte, and an excellent *exciting* electrolyte in proper cases (1880.), producing most powerful currents when it acts chemically; it is in all these points quite unlike the metals, and, in its action, like any of the acid or saline exciting electrolytes commonly used. How then can it be allowed that, without a single direct experiment, and solely for the purpose of avoiding the force of those which are placed in opposition, we should suppose it to leave its own station amongst the electrolytes, and class with the metals; and that too, in a point of character, which, even with them, is as yet a mere assumption (1809.)?

1862. But it is not with the sulphuret of potassium alone that this freedom must be allowed; it must be extended to the nitrous acid (1843. 1847.), to the nitric acid (1849, &c.), and even to the solution of potash (1854.); all these being of the class of electrolytes, and yet exhibiting no current in circuits where they do not occasion chemical action. Further, this exception must be made for *weak solutions* of sulphuret of potassium (1842.) and of potassa (1856.), for they exhibit the same phenomena as the stronger solutions. And if the contact theorists claim it for these weak solutions, then how will they meet the case of weak nitric acid which is not similar in its action on iron to strong nitric acid (1977.), but can produce a powerful current?

1863. The chemical philosopher is embarrassed by none of these difficulties; for he first, by a simple direct experiment, ascertains whether any of the two given substances in the circuit are active chemically on each other. If they are, he expects and finds the corresponding current; if they are not, he expects and he finds no current, though the circuit be a good conductor and he look carefully for it (1829.).

1864. Again; taking the case of iron, platina, and solution of sulphuret of potassium, there is no current; but for iron substitute zinc, and there is a powerful current. I might for zinc substitute copper, silver, tin, cadmium, bismuth, lead, and other metals; but I take zinc, because its sulphuret dissolves and is carried off by the solution, and so leaves the case in a very simple state; the fact, however, is as strong with any of the other metals. Now if the contact theory be true, and if the iron, platina, and solution of sulphuret of potassium give contacts which are in perfect equilibrium as to their electromotive force, then why does changing the iron for zinc destroy the equilibrium? Changing one metal for another in a metallic circuit causes no alteration of this kind: nor does changing one substance for another among the great number of bodies which, as solid conductors, may be used to form conducting (but chemically inactive) circuits (1867, &c.). If the solution of sulphuret of potassium is to be classed with the metals as to its action in the experiments I have quoted (1825, &c.), then, how comes it to act quite unlike them, and with a power equal to the *best* of the other class, in the new cases of zinc, copper, silver, &c. (1882. 1885, &c.)?

1865. This difficulty, as I conceive, must be met, on the part of the contact theorists, by a new assumption, namely, that this fluid sometimes acts as the best of the metals, or first class of conductors, and sometimes as the best of the electrolytes or second class. But surely this would be far too loose a method of philosophizing in an experimental science (1889.); and further, it is most unfortunate for such an assumption, that this second condition or relation of it never comes on by itself, so as to give us a pure case of a current from contact alone; it never comes on *without* that chemical action to which the chemist so simply refers all the current which is then produced.

1866. It is unnecessary for me to say that the same argument applies with equal force to the cases where nitrous acid, nitric acid, and solution of potash are used; and it is supported with equal strength by the results which they have given (1843. 1849. 1853.).

1867. It may be thought that it was quite unnecessary, but in my desire to establish contact electromotive force, to do which I was at one time very anxious, I made many circuits of three substances, including a galvanometer, all being conductors, with the hope of finding an arrangement, which, without chemical action, should produce a current. The number and variety of these experiments may be understood from the following summary; in which metals, plumbago, sulphurets and oxides, all being conductors even of a thermo current, were thus combined in various ways:

1. Platinum.
2. Iron.
3. Zinc.
4. Copper.
5. Plumbago.
6. Scale oxide of iron.
7. Native peroxide of manganese.
8. Native gray sulphuret of copper.
9. Native iron pyrites.
10. Native copper pyrites.
11. Galena.
12. Artificial sulphuret of copper.
13. Artificial sulphuret of iron.
14. Artificial sulphuret of bismuth.

1 and 2 with 5, 6, 7, 8, 9, 10, 11, 12, 13, 14, in turn.
1 and 3 with 5, 6, 7, 8, 9, 10, 11, 12, 13, 14.
1 and 5 with 6, 7, 8, 9, 10, 11, 12, 13, 14.
3 and 6 with 7, 8, 9, 10, 11, 12, 13, 14.
4 and 5 with 6, 7, 8, 9, 10, 11, 12, 13, 14.
4 and 6 with 7, 8, 9, 10, 11, 12, 13, 14.
4 and 7 with 8, 9, 10, 11, 12, 13, 14.
4 and 8 with 9, 10, 11, 12, 13, 14.
4 and 9 with 10, 11, 12, 13, 14.
4 and 10 with 11, 12, 13, 14.

4 and 11 with 12, 13, 14.
4 and 12 with 13, 14.
4 and 13 with 14.
1 and 4 with 12.

1868. Marianini states from experiment that copper is positive to sulphuret of copper[1]: with the Voltaists, according to the same philosopher, sulphuret of copper is positive to iron (1878.), and with them also iron is positive to copper. These three bodies therefore ought to give a most powerful circle: but on the contrary, whatever sulphuret of copper I have used, I have found not the slightest effect from such an arrangement.

1869. As peroxide of lead is a body causing a powerful current in solution of sulphuret of potassium, and indeed in every case of a circuit where it can give up part of its oxygen, I thought it reasonable to expect that its contact with metals would produce a current, if contact ever could. A part of that which had been prepared (1822.), was therefore well dried, which is quite essential in these cases, and formed into the following combinations:

Platinum.	Zinc.	Peroxide of lead.
Platinum.	Lead.	Peroxide of lead.
Platinum.	Cadmium.	Peroxide of lead.
Platinum.	Iron.	Peroxide of lead.

Of these varied combinations, not one gave the least signs of a current, provided differences of temperature were excluded; though in every case the circle formed was, as to conducting power, perfect for the purpose, i. e. able to conduct even a very weak thermo current.

1870. In the contact theory it is not therefore the metals alone that must be assumed to have their contact forces so balanced as to produce, in any circle of them, an effect amounting to nothing (1809.); but all solid bodies that are able to conduct, whether they be forms of carbon, or oxides, or sulphurets, must be included in the same category. So also must the electrolytes already referred to, namely, the solutions of sulphuret of potassium and potash, and nitrous and nitric acids, in every case where they do not act chemically. In fact *all*

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[1] Memoria della Società Italiana in Modena, 1827, xxi. 224.

conductors that do not act chemically in the circuit must be assumed, by the contact theory, to be in this condition, until a case of voltaic current without chemical action is produced (1858.).

1871. Then, even admitting that the results obtained by Volta and his followers with the electrometer prove that mere contact has an electromotive force and can produce an effect, surely all experience with contact alone goes to show that the electromotive forces in a circuit are always balanced. How else is it likely that the above-named most varied substances should be found to agree in this respect? unless indeed it be, as I believe, that all substances agree in this, of having no such power at all. If so, then where is the source of power which can account by the theory of contact for the current in the voltaic pile? If they are not balanced, then where is the sufficient case of contact alone producing a current? or where are the numerical data which indicate that such a case can be (1808. 1868.)? The contact philosophers are bound to produce, not a case where the current is infinitesimally small, for such cannot account for the current of the voltaic pile, and will always come within the debatable ground which De la Rive has so well defended, but a case and data of such distinctness and importance as may be worthy of opposition to the numerous cases produced by the chemical philosopher (1892.); for without them the contact theory as applied to the pile appears to me to have *no* support, and, as it asserts contact electromotive force even *with* the balanced condition, to be almost without foundation.

1872. To avoid these and similar conclusions, the contact theory must bend about in the most particular and irregular way. Thus the contact of solution of sulphuret of potassium with iron must be considered as balanced by the joint force of its contact with platinum, and the contact of iron and platinum with each other; but changing the iron for lead, then the contact of the sulphuret with the latter metal is no longer balanced by the other two contacts, it has all of a sudden changed its relation: after a few seconds, when a film of sulphuret has been formed by the chemical action, then the current ceases, though the circuit be a good conductor (1885.); and now it must be assumed that the solution has acquired its first relation to the metals and to the sulphuret of lead, and gives an equilibrium condition of the contacts in the circle.

1873. So also with this sulphuretted solution and with potassa, dilution must, by the theory, be admitted as producing *no change* in the character of the contact force; but with nitric acid, it, on the contrary, must be allowed to change the character of the force greatly (1977.). So again acids and alkalies (as potassa) in the cases where the currents are produced by them, as with zinc and platinum for instance, must be assumed as giving the preponderance of electromotive force on the same side, though these are bodies which might have been expected to give opposite currents, since they differ so much in their nature.

1874. Every case of a current is obliged to be met, on the part of the contact advocates, by assuming powers at the points of contact, in *the particular case*, of such proportionate strengths as will consist with the results obtained, and the theory is made to bend about (1956. 1992. 2006. 2014. 2063.), having no general relation for the acids or alkalies, or other electrolytic solution used. The result therefore comes to this: The theory can predict nothing regarding the results; it is accompanied by no case of a voltaic current produced without chemical action, and in those associated with chemical action, it bends about to suit the real results, these contortions being exactly parallel to the variations which the pure chemical force, by experiment, indicates.

1875. In the midst of all this, how simply does the chemical theory meet, include, combine, and even predict, the numerous experimental results! When there is a current there is also chemical action; when the action ceases, the current stops (1882. 1885. 1894.); the action is determined either at the anode or the cathode, according to circumstances (2039. 2041.), and the direction of the current is invariably associated with the direction in which the active chemical forces oblige the anions and cations to move in the circle (962. 2052.).

1876. Now when in conjunction with these circumstances it is considered, that the many arrangements without chemical action (1825, &c.) produce no current; that those with chemical action almost always produce a current; that hundreds occur in which chemical action without contact produces a current (2017, &c.); and that as many with contact but without chemical action (1867.) are known and are inactive; how can we resist the conclusion, that the powers of the voltaic battery originate in the exertion of chemical force?

¶ iii. *Active circles excited by solution of sulphuret of potassium.*

1877. In 1812 Davy gave an experiment to show, that of two different metals, copper and iron, that having the strongest attraction for oxygen was positive in oxidizing solutions, and that having the strongest attraction for sulphur was positive in sulphuretting solutions[1]. In 1827 De la Rive quoted several such inversions of the states of two metals, produced by using different solutions, and reasoned from them, that the mere contact of the metals could not be the cause of their respective states, but that the chemical action of the liquid produced these states[2].

1878. In a former paper I quoted Sir Humphry Davy's experiment (943.), and gave its result as a proof that the contact of the iron and copper could not originate the current produced; since when a dilute acid was used in place of the sulphuret, the current was reverse in direction, and yet the contact of the metals remained the same. M. Marianini[3] adds, that copper will produce the same effect with tin, lead, and even zinc; and also that silver will produce the same results as copper. In the case of copper he accounts for the effect by referring it to the relation of the iron and the new body formed on the copper, the latter being, according to Volta, positive to the former[4]. By his own experiment the same substance was negative to the iron across the same solution[5].

1879. I desire at present to resume the class of cases where a solution of sulphuret of potassium is the liquid in a voltaic circuit; for I think they give most powerful proof that the current in the voltaic battery cannot be produced by contact, but is due altogether to chemical action.

1880. The solution of sulphuret of potassium (1812.) is a most excellent conductor of electricity (1814.). When subjected between platinum electrodes to the decomposing power of a small voltaic battery, it readily gave pure sulphur at the anode, and a little gas, which was probably hydrogen, at the cathode. When arranged with platinum surfaces so as to form a Ritter's secondary pile, the passage of a feeble primary current, for a few seconds only, makes this secondary battery effective in causing a counter current; so that, in accordance with electrolytic conduction (923. 1343.), it probably does not conduct without decomposition, or if at all, its point of electrolytic intensity (966. 983.) must be very low. Its exciting action (speaking on the chemical theory) is either the giving an anion (sulphur) to such metallic and other bodies as it can act upon, or, in some cases, as with the peroxides of lead and manganese, and the protoxide of iron (2046.), the abstraction of an anion *from* the body in contact with it, the current produced being in the one or the other direction accordingly. Its chemical affinities are such, that in many cases its anion goes to that metal, of a pair of metals, which is left untouched when the usual exciting electrolytes are employed; and so a beautiful inversion of the current in relation to the metals is obtained; thus, when copper and nickel are used with it, the anion goes to the copper; but when the same metals are used with the ordinary electrolytic fluids, the anion goes to the nickel. Its excellent conducting power renders the currents it can excite very evident and strong; and it should be remembered that the strength of the resulting currents, as indicated by the galvanometer, depends jointly upon the energy (not the mere quantity) of the exciting action called into play, and the conductive ability of the circuit through which the current has to run. The value of this exciting electrolyte is increased for the present investigation, by the circumstance of its giving, by its action on the metals, resulting compounds, some of which are insoluble, whilst others are soluble; and, of the insoluble results, some are excellent conductors, whilst others have no conducting power at all.

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[1] Elements of Chemical Philosophy, p. 148.

[2] Annales de Chimie, 1828, xxxvii. 231–237; xxxix. 299.

[3] Memorie della Società Italiana in Modena, 1837, xxi. p. 224.

[4] Ibid. p. 219.

[5] Ibid. p. 224.

1881. The experiments to be described were made generally in the following manner. Wires of platinum, gold, palladium, iron, lead, tin, and the other malleable metals, about one twentieth of an inch in diameter and six inches long, were prepared.

Two of these being connected with the ends of the galvanometer-wires, were plunged at the same instant into the solution of sulphuret of potassium in a test-glass, and kept there without agitation (1919.), the effects at the same time being observed. The wires were in every case carefully cleansed with fresh fine sand-paper and a clean cloth ; and were sometimes even burnished by a glass rod, to give them a smooth surface. Precautions were taken to avoid any difference of temperature at the junctions of the different metals with the galvanometer-wires.

1882. *Tin and platinum.*—When tin was associated with platinum, gold, or, I may say, any other metal which is chemically inactive in the solution of the sulphuret, a strong electric current was produced, the tin being positive to the platinum through the solution, or, in other words, the current being from the tin through the solution to the platinum. In a very short time this current fell greatly in power, and in ten minutes the galvanometer-needle was nearly at 0°. On then endeavouring to transmit the antimony-bismuth thermo current (1825.) through the circuit, it was found that it could not pass, the circle having lost its conducting power. This was the consequence of the formation on the tin of an insoluble, investing, non-conducting sulphuret of that metal; the non-conducting power of the body formed is not only evident from the present result, but also from a former experiment (1821.).

1883. Marianini thinks it is possible that (in the case of copper, at least (1878.), and, so I presume, for all similar cases, for surely one law or principle should govern them,) the current is due to the contact force of the sulphuret formed. But that application is here entirely excluded ; for how can a *non-conducting* body form a current, either by contact or in any other way? No such case has ever been shown, nor is it in the nature of things ; so that it cannot be the contact of the sulphuret that here causes the current; and if not in the present, why in any case? for nothing happens here that does not happen in any other instance of a current produced by the same exciting electrolyte.

1884. On the other hand, how beautiful a proof the result gives in confirmation of the chemical theory! Tin can take sulphur from the electrolyte to form a sulphuret; and whilst it is doing so, and in proportion to the degree in which it is doing so, it produces a current; but when the sulphuret which is formed, by investing the metal, shuts off the fluid and prevents further chemical action, then the current ceases also. Nor is it *necessary* that it should be a non-conductor for this purpose, for conducting sulphurets will perform the same office (1885. 1894.), and bring about the same result. What, then, can be more clear, than that whilst the sulphuret is *being formed* a current is produced, but that when formed its mere contact can do nothing towards such an effect?

1885. *Lead.*—This metal presents a fine result in the solution of sulphuret of potassium. Lead and platinum being the metals used, the lead was at first highly positive, but in a few seconds the current fell, and in two minutes the galvanometer-needle was at 0°. Still the arrangement conducted a feeble thermo current extremely well, the conducting power not having disappeared, as in the case of tin; for the investing sulphuret of lead is a conductor (1820.). Nevertheless, though a conductor, it could stop the further chemical action; and that ceasing, the current ceased also.

1886. Lead and gold produced the same effect. Lead and palladium the same. Lead and iron the same, except that the circumstances respecting the tendency of the latter metal under common circumstances to produce a current from the electrolyte to itself, have to be considered and guarded against (1826. 2049.). Lead and nickel also the same. In all these cases, when the lead was taken out and washed, it was found beautifully invested with a thin polished pellicle of sulphuret of lead.

1887. With lead, then, we have a *conducting* sulphuret formed, but still there is no sign that its contact can produce a current, any more than in the case of the *non-conducting* sulphuret of tin (1882.). There is no new or additional action produced by this *conducting* body; there was no deficiency of action with the former *non-conducting* product; both are alike in their results, being, in fact, essentially alike in their relation to that on which the current really depends, namely, an active chemical force. A piece of lead put *alone* into the solution of sulphuret of potassium, has its surface converted into sulphuret of lead, the proof thus being obtained, even when the current cannot be formed, that there is a force (chemical) present and active under such circumstances; and such force can produce a current of chemical force when the circuit form is given to the arrangement. The force at the place of excitement shows itself, both by the formation of sulphuret of lead and the production of a current. In proportion as the formation of the one decreases the production of the other diminishes, though all the bodies produced are conductors, and contact still remains to perform any work or cause any effect to which it is competent.

1888. It may perhaps be said that the current is due to the contact between the solution of sulphuret and the lead, (or tin, as the case may be,) which occurs at the beginning of the experiment; and that when the action ceases, it is because a new body, the sulphuret of lead, is introduced into the circuit, the various contacts being then balanced in their force. This would be to fall back upon the assumption before resisted (1861. 1865. 1872.), namely, that the solution may class with metals and such like bodies, giving balanced effects of contact in relation to *some* of these bodies, as in this case, to the sulphuret of lead produced, but not with *others*, as the lead itself; both the lead and its sulphuret being in the same category as the metals generally (1809. 1870.).

1889. The utter improbability of this as a natural effect, and the absence of all experimental proof in support of it, have been already stated (1861. 1871.), but one or two additional reasons against it now arise. The state of things may perhaps be made clearer by a diagram or two, in which assumed contact forces may be assigned, in the absence of all experimental expression, without injury to the reasoning. Let fig. 4, Plate III. represent the electromotive forces of a circle of platinum, iron, and solution of sulphuret of potassium; or platinum, nickel, and solution of sulphuret; cases in which the forces are, according to the contact theory, balanced (1860.). Then fig. 5 may represent the circle of platinum, lead, and solution of sulphuret, which does produce a current, and, as I have assumed, with a resulting force of 11 ⟶. This in a few minutes becomes quiescent, i. e. the current ceases, and fig. 6 may represent this new case according to the contact theory. Now is it at all likely that by the intervention of sulphuret of lead at the contact *c*, fig. 5, and the production of two contacts *d* and *e*, fig. 6, such an enormous change of the contact force suffering alteration should be made as from 10 to 21? the intervention of the same sulphuret either at *a* or *b* (1834. 1840.) being able to do nothing of the kind, for the sum of the force of the two new contacts is in that case exactly equal to the force of the contact which they replace, as is proved by such interposition making no change in the effects of the circle (1867. 1840.). If therefore the intervention of this body between *lead* and platinum at *a*, or between solution of *sulphuret of potassium* and platinum at *b* (fig. 5) causes no change, these cases including its contact with both lead and the solution of sulphuret, is it at all probable that its intervention between these two bodies at *c* should make a difference equal to double the amount of force previously existing, or indeed any difference at all?

1890. Such an alteration as this in the sum assigned as the amount of the forces belonging to the sulphuret of lead by virtue of its two places of contact, is equivalent I think to saying that it partakes of the anomalous character already supposed to belong to certain fluids, namely, of sometimes giving balanced forces in circles of good conductors, and at other times not (1865.).

1891. Even the metals themselves must in fact be forced into this constrained condition; for the effect at a point of contact, if there be *any at all*, must be the result of the *joint* and *mutual actions* of the bodies in contact. If therefore in the circuit, fig. 5, the contact forces are not balanced, it must be because of the deficient *joint* action of the lead and solution at *c*[1]. If the metal and fluid were to act in their proper character, and as iron or nickel would do in the place of the lead, then the force there would be ←—— 21, whereas it is less, or according to the assumed numbers only ←—— 10. Now as there is no reason why the lead should have any superiority assigned to it over the solution, since the latter can give a balanced condition amongst good conductors in its proper situation as well as the former; how can this be, unless lead possess that strange character of sometimes giving equipoised contacts, and at other times not (1865.)?

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[1] My numbers are assumed, and if other numbers were taken, the reasoning might be removed to contact *b*, or even to contact *a*, but the end of the argument would in every case be the same.

1892. If that be true of lead, it must be true of all the metals which, with this sulphuretted electrolyte, give circles producing currents; and this would include bismuth, copper, antimony, silver, cadmium, zinc, tin, &c. &c. With other electrolytic fluids iron and nickel would be included, and even gold, platinum, palladium; in fact all the bodies that can be made to yield in any way active voltaic circuits. Then is it possible that this can be true, and yet not a single combination of this extensive class of bodies be producible that can give the current without chemical action (1867.), considered not as a result, but as a known and pre-existing force?

1893. I will endeavour to avoid further statement of the arguments, but think myself bound to produce (1799.) a small proportion of the enormous body of facts which appear to me to bear evidence all in one direction.

1894. *Bismuth.*—This metal, when associated with platinum, gold, or palladium in solution of the sulphuret of potassium, gives active circles, the bismuth being positive. In the course of less than half an hour the current ceases; but the circuit is still an excellent conductor of thermo currents. Bismuth with iron or nickel produces the same final result with the reservation before made (1826.). Bismuth and lead give an active circle; at first the bismuth is positive; in a minute or two the current ceases, but the circuit still conducts the thermo current well.

1895. Thus whilst sulphuret of bismuth is in the act of formation the current is produced; when the chemical action ceases the current ceases also; though contact continues and the sulphuret be a good conductor. In the case of bismuth and lead the chemical action occurs at both sides, but is most energetic at the bismuth, and the current is determined accordingly. Even in that instance the cessation of chemical action causes the cessation of the current.

1896. In these experiments with *lead* and *bismuth* I have given their associations with platinum, gold, palladium, iron, and nickel; because, believing in the first place that the results prove all current to depend on chemical action, then, the quiescent state of the resulting or final circles shows that the contacts of these metals in their respective pairs are *without force* (1829.): and upon that again follows the passive condition of all those contacts which can be produced by interposing other conducting bodies between them (1833.); an argument that need not again be urged.

1897. *Copper.*—This substance being associated with platinum, gold, iron, or any metal chemically inactive in the solution of sulphuret, gives an active circle, in which the copper is positive through the electrolyte to the other metal. The action, though it falls, does not come to a close as in the former cases, and for these simple reasons; that the sulphuret formed is not compact but porous, and does not adhere to the copper, but separates from it in scales. Hence results a continued renewal of the chemical action between the metal and electrolyte, and a continuance of the current. If after a while the copper plate be taken out and washed, and dried, even the wiping will remove part of the sulphuret in scales, and the nail separates the rest with facility. Or if a copper plate be left in abundance of the solution of sulphuret, the chemical action *continues*, and the coat of sulphuret of copper becomes thicker and thicker.

1898. If, as Marianini has shown[1], a copper plate which has been dipped in the solution of sulphuret, be removed before the coat formed is so thick as to break up from the metal beneath, and be washed and dried, and then replaced, in association with platinum or iron, in the solution, it will at first be neutral, or, as is often the case, negative (1827. 1838.) to the other metal, a result quite in opposition to the idea, that the mere presence of the sulphuret on it could have caused the former powerful current and positive state of the copper (1897. 1878.). A further proof that it is not the mere *presence*, but the *formation*, of the sulphuret which causes the current, is, that, if the plate be left long enough for the solution to penetrate the investing crust of sulphuret of copper and come into activity on the metal beneath, then the plate becomes active, and a current is produced.

1899. I made some sulphuret of copper, by igniting thick copper wire in a Florence flask or crucible in abundance of vapour of sulphur. The body produced is in an excellent form for these experiments, and a good conductor; but it is not without action on the sulphuretted solution, from which it can take more sulphur, and the consequence is, that it is positive to platinum or iron in such a solution. If such sulphuret of copper be left long in the solution, and then be washed and dried, it will generally acquire the final state of sulphuration, either in parts or altogether, and also be inactive, as the sulphuret formed on the copper was before (1898.); i. e. when its chemical action is exhausted, it ceases to produce a current.

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[1] Memorie della Società Italiana in Modena, 1837, xxi. 224.

1900. *Native gray sulphuret of copper* has the same relation to the electrolyte: it takes sulphur from it and is raised to a higher state of combination; and, as it is also a conductor (1820.), it produces a current, being itself positive so long as the action continues.

1901. But when the copper is *fully sulphuretted,* then all these actions cease; though the sulphuret be a conductor, the contacts still remain, and the circle can carry with facility a feeble thermo current. This is not only shown by the quiescent cases just mentioned (1898.), but also by the utter inactivity of platinum and *compact yellow copper pyrites,* when conjoined by this electrolyte, as shown in a former part of this paper (1840.).

1902. *Antimony.*—This metal, being put alone into a solution of sulphuret of potassium, is acted on, and a sulphuret of antimony formed which does not adhere strongly to the metal, but wipes off. Accordingly, if a circle be formed of antimony, platinum, and the solution, the antimony is positive in the electrolyte, and a powerful current is formed, which continues. Here then is another beautiful variation of the conditions under which the chemical theory can so easily account for the effects, whilst the theory of contacts cannot. The sulphuret produced in this case is a non-conductor whilst in the solid state (402.); it cannot therefore be that any contact of this sulphuret can produce the current; in that respect it is like the sulphuret of tin (1882.). But that circumstance does not stop the occurrence of the chemical current; for, as the sulphuret forms a porous instead of a continuous crust, the electrolyte has access to the metal and the action goes on.

1903. *Silver.*—This metal, associated with platinum, iron, or other metals inactive in this electrolyte, is strongly positive, and gives a powerful continuous current. Accordingly, if a plate of silver, coated with sulphuret by the simple action of the solution, be examined, it will be found that the crust is brittle and broken, and separates almost spontaneously from the metal. In this respect, therefore, silver and copper are alike, and the action consequently continues in both cases; but they differ in the sulphuret of silver being a non-conductor (434.) for these feeble currents, and, in that respect, this metal is analogous to antimony (1902.).

1904. *Cadmium.*—Cadmium with platinum, gold, iron, &c., gives a powerful current in the solution of sulphuret, and the cadmium is positive. On several occasions this current continued for two or three hours or more; and at such times, the cadmium being taken out, washed and wiped, the sulphuret was found to separate easily in scales on the cloth used.

1905. Sometimes the current would soon cease; and then the circle was found not to conduct the thermo current (1813.). In these cases, also, on examining the cadmium, the coat of sulphuret was strongly adherent, and this was more especially the case when prior to the experiment the cadmium, after having been cleaned, was burnished by a glass rod (1881.). Hence it appears that the sulphuret of this metal is a non-conductor, and that its contact could not have caused the current (1883.) in the manner Marianini supposes. All the results it supplies are in perfect harmony with the chemical theory and adverse to contact theory.

1906. *Zinc.*—This metal, with platinum, gold, iron, &c., and the solution of sulphuret, produces a very powerful current, and is positive through the solution to the other metal. The current was permanent. Here another beautiful change in the circumstances of the general experiment occurs. Sulphuret of zinc is a non-conductor of electricity (1821.), like the sulphurets of tin, cadmium, and antimony; but then it is soluble in the solution of sulphuret of potassium; a property easily ascertainable by putting a drop of solution of zinc into a portion of the electrolytic solution, and first stirring them a little, by which abundance of sulphuret of zinc will be formed; and then stirring the whole well together, when it will be redissolved. The consequence of this solubility is, that the zinc when taken out of the solution is perfectly free from investing sulphuret of zinc. Hence, therefore, a very sufficient reason, on the chemical theory, why the action should go on. But how can the theory of contact refer the current to any contact of the metallic sulphuret, when that sulphuret is, in the first place, a non-conductor, and, in the next, is dissolved and carried off into the solution at the moment of its formation?

1907. Thus all the phenomena with this admirable electrolyte (1880.), whether they be those which are related to it as an active (1879.) or as a passive (1825, &c.) body, confirm the chemical theory, and oppose that of contact. With tin and cadmium it gives an impermeable non-conducting body; with lead and bismuth it gives an impermeable conducting body; with antimony and silver it produces a permeable non-conducting body; with copper a permeable conducting body; and with zinc a soluble non-conducting body. The chemical action and its resulting current are perfectly consistent with all these variations. But try to explain them by the theory of contact, and, as far as I can perceive, that can only be done by twisting the theory about and making it still more tortuous than before (1861. 1865. 1872. 1874. 1889.); special assumptions being necessary to account for the effects which, under it, become so many special cases.

1908. *Solution of protosulphuret of potassium, or bihydrosulphuret of potassa.*—I used a solution of this kind as the electrolyte in a few cases. The results generally were in accordance with those already given, but I did not think it necessary to pursue them at length. The solution was made by passing sulphuretted hydrogen gas for twenty-four hours through a strong solution of pure caustic potassa.

1909. Iron and platinum with this solution formed a circle in which the iron was first negative, then gradually became neutral, and finally acquired a positive state. The solution first acted as the yellow sulphuret in reducing the investing oxide (2049.), and then, apparently, directly on the iron, dissolving the sulphuret formed. Nickel was positive to platinum from the first, and continued so though producing only a weak current. When weak chemical action was substituted for metallic contact at *x*, fig. 2 (1831.), a powerful current passed. Copper was highly positive to iron and nickel; as also to platinum, gold, and the other metals which were unacted upon by the solution. Silver was positive to iron, nickel, and even lead; as well as to platinum, gold, &c. Lead is positive to platinum, then the current falls, but does not cease. Bismuth is also positive at first, but after a while the current almost entirely ceases, as with the yellow sulphuret of potassium (1894.).

1910. Native gray sulphuret of copper and artificial sulphuret of copper (1899.) were positive to platinum and the inactive metals: but yellow copper pyrites, yellow iron pyrites, and galena, were inactive with these metals in this solution; as before they had been with the solution of yellow or bisulphuret of potassium. This solution, as might be expected from its composition, has more of alkaline characters in it than the yellow sulphuret of potassium.

1911. Before concluding this account of results with the sulphuretted solutions, as exciting electrolytes, I will mention the varying and beautiful phenomena which occur when copper and silver, or two pieces of copper, or two pieces of silver, form a circle with the yellow solution. If the metals be copper and silver, the copper is at first positive and the silver remains untarnished; in a short time this action ceases, and the silver becomes positive; at the same instant it begins to combine with sulphur and becomes covered with sulphuret of silver; in the course of a few moments the copper again becomes positive; and thus the action will change from side to side several times, and the current with it, according as the circumstances become in turn more favourable at one side or the other.

1912. But how can it be thought that the current first produced is due in any way to the *contact* of the sulphuret of copper formed, since its presence there becomes at last the reason why that first current diminishes, and enables the silver, which is originally the weaker in exciting force, and has no sulphuret as yet formed on it, to assume for a time the predominance, and produce a current which can overcome that excited at the copper (1911.)? What can account for these changes, but chemical action? which, as it appears to me, accounts, as far as we have yet gone, with the utmost simplicity, for *all* the effects produced, however varied the mode of action and their circumstances may be.

Royal Institution,
December 12, 1839.

SEVENTEENTH SERIES.

§ 24. *On the source of power in the voltaic pile.*—(Continued.) ¶ iv. *The exciting chemical force affected by temperature.* ¶ v. *The exciting chemical force affected by dilution.* ¶ vi. *Differences in the order of the metallic elements of voltaic circles.* ¶ vii. *Active voltaic circles and batteries without metallic contact.* ¶ viii. *Considerations of the sufficiency of chemical action.* ¶ ix. *Thermo-electric evidence.* ¶ x. *Improbable nature of the assumed contact force.*

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¶ iv. *The exciting chemical force affected by temperature.*

1913. ON the view that chemical force is the origin of the electric current in the voltaic circuit, it is important that we have the power of causing by ordinary chemical means, a variation of that force within certain limits, without involving any alteration of the metallic or even the other contacts in the circuit. Such variations should produce corresponding voltaic effects, and it appeared not improbable that these differences alone might be made effective enough to produce currents without any metallic contact at all.

1914. De la Rive has shown that the increased action of a pair of metals, when put into hot fluid instead of cold, is in a great measure due to the exaltation of the chemical affinity on that metal which was acted upon[1]. My object was to add to the argument by using but one metal and one fluid, so that the fluid might be alike at both contacts, but to exalt the chemical force at one only of the contacts by the action of heat. If such difference produced a current with circles which either did not generate a thermo current themselves, or could not conduct that of an antimony and bismuth element, it seemed probable that the effect would prove to be a result of pure chemical force, contact doing nothing.

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[1] Annales de Chimie, 1828, xxxvii. p. 242.

1915. The apparatus used was a glass tube (Plate III. fig. 7.) about five inches long and 0·4 of an inch internal diameter, open at both ends, bent, and supported on a retort-stand. In this the liquid was placed, and the portion in the upper part of one limb could then easily be heated and retained so, whilst that in the other limb was cold. In the experiments I will call the left-hand side A, and the right-hand side B, taking care to make no change of these designations. C and D are the wires of metal (1881.) to be compared; they were formed into a circuit by means of the galvanometer, and, often also, a Seebeck's thermo-element of antimony and bismuth; both these, of course, caused no disturbing effect so long as the temperature of their various junctions was alike. The wires were carefully prepared (1881.), and when two of the same metal were used, they consisted of the successive portions of the same piece of wire.

1916. The precautions which are necessary for the elimination of a correct result are rather numerous, but simple in their nature.

1917. *Effect of first immersion.*—It is hardly possible to have the two wires of the same metal, even platinum, so exactly alike that they shall not produce a current in consequence of their difference; hence it is necessary to alternate the wires and repeat the experiment several times, until an undoubted result independent of such disturbing influences is obtained.

1918. *Effect of the investing fluid or substance.*—The fluid produced by the action of the liquid upon the metal exerts, as is well known, a most important influence on the production of a current. Thus when two wires of cadmium were used with the apparatus, fig. 7, (1915.) containing dilute sulphuric acid, hot on one side and cold on the other, the hot cadmium was at first positive, producing a deflection of about 10°; but in a short time this effect disappeared, and a current in the reverse direction equal to 10° or more would appear, the hot cadmium being now negative. This I refer to the quicker exhaustion of the chemical forces of the film of acid on the heated metallic surface (1003. 1036. 1037.), and the consequent final superiority of the colder side at which the action was thus necessarily more powerful (1953, &c. 1966. 2015. 2031, &c.). Marianini has described many cases of the effects of investing solutions, showing that if two pieces of the same metal (iron, tin, lead, zinc, &c.) be used, the one first immersed is negative to the other, and has given his views of the cause[1]. The precaution against this effect was not to put the metals into the acid until the proper temperature had been given to both parts of it, and then to observe the *first effect* produced, accounting that as the true indication, but repeating the experiment until the result was certain.

1919. *Effect of motion.*—This investing fluid (1918.) made it necessary to guard against the effect of successive rest and motion of the metal in the fluid. As an illustration, if two tin wires (1881.) be put into dilute nitric acid, there will probably be a little motion at the galvanometer, and then the needle will settle at 0°. If either wire be then moved, the other remaining quiet, that in motion will become positive. Again, tin and cadmium in dilute sulphuric acid gave a strong current, the cadmium being positive, and the needle was deflected 80°. When left, the force of the current fell to 35°. If the cadmium were then moved it produced very little alteration; but if the tin were moved it produced a great change, not showing, as before, an increase of its force, but the reverse, for it became more negative, and the current force rose up again to 80°[2]. The precaution adopted to avoid the interference of these actions, was not only to observe the first effect of the introduced wires, but to keep them moving from the moment of the introduction.

1920. The above effect was another reason for heating the acids, &c. (1918.) before the wires were immersed; for in the experiment just described, if the cadmium side were heated to boiling, the moment the fluid was agitated on the tin side by the boiling on the cadmium side, there was more effect by far produced by the motion than the heat: for the heat at the cadmium alone did little or nothing, but the jumping of the acid over the tin made a difference in the current of 20° or 30°.

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[1] Annales de Chimie, 1830, xlv. p. 40.

[2] Tin has some remarkable actions in this respect. If two tins be immersed in succession into dilute nitric acid, the one last in is positive to the other at the moment: if, both being in, one be moved, that is for the time positive to the other. But if dilute sulphuric acid be employed, the last tin is always negative: if one be taken out, cleaned, and reimmersed, it is negative: if, both being in and neutral, one be moved, it becomes negative to the other. The effects with muriatic acid are the same in kind as those with sulphuric acid, but not so strong. This effect perhaps depends upon the compound of tin first produced in the sulphuric and muriatic acids tending to acquire some other and more advanced state, either in relation to the oxygen, chlorine or acid concerned, and so adding a force to that which at the first moment, when only metallic tin and acid are present, tends to determine a current.

1921. *Effect of air.*—Two platinum wires were put into cold strong solution of sulphuret of potassium (1812.), fig. 7; and the galvanometer was soon at 0°. On heating and boiling the fluid on the side A (1915.) the platinum in it became negative; cooling that side, by pouring a little water over it from a jug, and heating the side B, the platinum there in turn became negative; and, though the action was irregular, the same general result occurred however the temperatures of the parts were altered. This was not due to the chemical effect of the electrolyte on the heated platinum. Nor do I believe it was a true thermo current (1933.); but if it were the latter, then the heated platinum was *negative* through the electrolyte to the cold platinum. I believe it was altogether the increased effect of the air upon the electrolyte at the heated side; and it is evident that the application of the heat, by causing currents in the fluid and also in the air, facilitates their mutual action at that place. It has been already shown, that lifting up a platinum wire in this solution, so as to expose it for a moment to the air (1827.), renders it negative when reimmersed, an effect which is in perfect accordance with the assumed action of the heated air and fluid in the present case. The interference of this effect is obviated by raising the temperature of the electrolyte quietly before the wires are immersed (1918.), and observing only the first effect.

1922. *Effect of heat.*—In certain cases where two different metals are used, there is a very remarkable effect produced on heating the negative metal. This will require too much detail to be described fully here; but I will briefly point it out and illustrate it by an example or two.

1923. When two platinum wires were compared in hot and cold dilute sulphuric acid (1935.), they gave scarcely a sensible trace of any electric current. If any real effect of heat occurred, it was that the hot metal was the least degree positive. When silver and silver were compared, hot and cold, there was also no sensible effect. But when platinum and silver were compared in the same acid, different effects occurred. Both being cold, the silver in the A side fig. 7 (1915.) was positive about 4°, by the galvanometer; moving the platina on the other side B did not alter this effect, but on heating the acid and platinum there, the current became very powerful, deflecting the needle 30°, and the silver was positive. Whilst the heat continued, the effect continued; but on cooling the acid and platinum it went down to the first degree. No such effect took place at the silver; for on heating that side, instead of becoming negative, it became more positive, but only to the degree of deflecting the needle 16°. Then, *motion* of the platinum (1919.) facilitated the passing of the current and the deflection increased, but *heating* the platinum side did far more.

1924. *Silver and copper* in dilute sulphuric acid produced very little effect; the copper was positive about 1° by the galvanometer; moving the copper or the silver did nothing; heating the copper side caused no change; but on heating the silver side it became negative 20°. On cooling the silver side this effect went down, and then, either moving the silver or copper, or heating the copper side, caused very little change: but heating the silver side made it negative as before.

1925. All this resolves itself into an effect of the following kind; that where two metals are in the relation of positive and negative to each other in such an electrolyte as dilute acids, (and perhaps others,) heating the negative metal at its contact with the electrolyte enables the current, which tends to form, to pass with such facility, as to give a result sometimes tenfold more powerful than would occur without it. It is not displacement of the investing fluid, for motion will in these cases do nothing: it is not chemical action, for the effect occurs at that electrode where the chemical action is not active; it is not a thermo-electric phenomenon of the ordinary kind, because it depends upon a voltaic relation; i. e. the metal showing the effect must be negative to the other metal in the electrolyte; so silver heated does nothing with silver cold, though it shows a great effect with copper either hot or cold (1924.); and platinum hot is as nothing to platina cold, but much to silver either hot or cold.

1926. Whatever may be the intimate action of heat in these cases, there is no doubt that it is dependent on the current which tends to pass round the circuit. It is essential to remember that the increased effect on the galvanometer is not due to any increase in the electromotive force, but solely to the removal of obstruction to the current by an increase probably of discharge. M. de la Rive has described an effect of heat, on the passage of the electric current, through dilute acid placed in the circuit, by platinum electrodes. Heat applied to the negative electrode increased the deflection of a galvanometer needle in the circuit, from 12° to 30° or 45°; whilst heat applied to the positive electrode caused no change[1]. I have not been able to obtain this nullity of effect at the positive electrode when a voltaic battery was used (1639.); but I have no doubt the present phenomena will prove to be virtually the same as those which that philosopher has described.

1927. The effect interferes frequently in the ensuing experiments when *two* metals, hot and cold, are compared with each other; and the more so as the negative metal approximates in inactivity of character to platinum or rhodium. Thus in the comparison of cold copper, with hot silver, gold, or platinum, in dilute nitric acid, this effect tends to make the copper appear more positive than it otherwise would do.

1928. *Place of the wire terminations.*—It is requisite that the *end* of the wire on the hot side should be *in* the heated fluid. Two copper wires were put into diluted solution of sulphuret of potassium, fig. 8; that portion of the liquid extending from C to D was heated, but the part between D and E remained cold. Whilst both ends of the wires were in the cold fluid, as in the figure, there were irregular movements of the galvanometer, small in degree, leaving the B wire positive. Moving the wires about, but retaining them as in the figure, made no difference; but on raising the wire in A, so that its termination should be in the hot fluid between C and D, then it became positive and continued so. On lowering the end into the cold part, the former state recurred; on raising it into the hot part, the wire again became positive. The same is the case with two silver wires in dilute nitric acid; and though it appears very curious that the current should increase in strength as the extent of bad conductor increases, yet such is often the case under these circumstances. There can be no reason to doubt that the part of the wire which is in the hot fluid at the A side, is at all times equally positive or nearly so; but at one time the whole of the current it produces is passing through the entire circuit by the wire in B, and at another, a part, or the whole, of it is circulating to the cold end of its own wire, only by the fluid in tube A.

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[1] Bibliothèque Universelle, 1837, vii. 388.

1929. *Cleaning the wires.*—That this should be carefully done has been already mentioned (1881.); but it is especially necessary to attend to the very extremities of the wires, for if these circular spaces, which occur in the most effective part of the circle, be left covered with the body produced on them in a preceding trial, an experimental result will often be very much deranged, or even entirely falsified.

1930. Thus the best mode of experimenting (1915.) is to heat the liquid in the limb A or B, fig. 8, first; and, having the wires well cleaned and connected, to plunge both in at once, and, retaining the *end* of the heated wire in the hot part of the fluid, to keep both wires in motion, and observe, especially, the first effects: then to take out the wires, reclean them, change them side for side and repeat the experiment, doing this so often as to obtain from the several results a decided and satisfactory conclusion.

1931. It next becomes necessary to ascertain whether any true thermo current can be produced by electrolytes and metals, which can interfere with any electro-chemical effects dependent upon the action of heat. For this purpose different combinations of electrolytes and metals not acted on chemically by them, were tried, with the following results.

1932. Platinum and a very *strong solution of potassa* gave, as the result of many experiments, the hot platinum positive across the electrolyte to the cold platinum, producing a current that could deflect the galvanometer needle about 5°, when the temperatures at the two junctures were 60° and 240°. Gold and the same solution gave a similar result. Silver and a moderately strong solution, of specific gravity 1070, like that used in the ensuing experiments (1948.) gave the hot silver positive, but now the deflection was scarcely sensible, and not more than 1°. Iron was tried in the same solution, and there was a constant current and deflection of 50° or more, but there was also chemical action (1948.).

1933. I then used *solution of the sulphuret of potassium* (1812.). As already said, hot platinum is negative in it to the cold metal (1921.); but I do not think the action was thermo-electric. Palladium with a weaker solution gave no indication of a current.

1934. Employing dilute nitric acid, consisting of one volume strong acid and fifty volumes water, platinum gave no certain indication: the hot metal was sometimes in the least degree positive, and at others an equally small degree negative. Gold in the same acid gave a scarcely sensible result; the hot metal was negative. Palladium was as gold.

1935. With dilute sulphuric acid, consisting of one by weight of oil of vitriol and eighty of water, neither platinum nor gold produced any sensible current to my galvanometer by the mere action of heat.

1936. *Muriatic acid* and platinum being conjoined, and heated as before, the hot platinum was very slightly negative in strong acid: in dilute acid there was no sensible current.

1937. *Strong nitric acid* at first seemed to give decided results. Platinum and pure strong nitric acid being heated at one of the junctions, the hot platinum became constantly negative across the electrolyte to the cold metal, the deflection being about 2°. When a yellow acid was used, the deflection was greater; and when a very orange-coloured acid was employed, the galvanometer needle stood at 70°, the hot platinum being still negative. This effect, however, is not a pure thermo current, but a peculiar result due to the presence of nitrous acid (1848.). It disappears almost entirely when a dilute acid is used (1934.); and what effect does remain indicates that the hot metal is negative to the cold.

1938. Thus the *potash solution* seems to be the fluid giving the most probable indications of a thermo current. Yet there the deflection is only 5°, though the fluid, being very strong, is a good conductor (1819.). When the fluid was diluted, and of specific gravity 1070, like that before used (1932.), the effect was only 1°, and cannot therefore be confounded with the results I have to quote.

1939. The dilute *sulphuric* (1935.) and *nitric* acids used (1934.) gave only doubtful indications in some cases of a thermo current. On trial it was found that the thermo current of an antimony-bismuth pair could not pass these solutions, as arranged in these and other experiments (1949. 1950.); that, therefore, if the little current obtained in the experiments be of a thermo-electric nature, this combination of platinum and acid is far more powerful than the antimony-bismuth pair of Seebeck; and yet that (with the interposed acid) it is scarcely sensible by this delicate galvanometer. Further, when there is a current, the hot metal is generally negative to the cold, and it is therefore impossible to confound these results with those to be described where the current has a contrary direction.

1940. In strong nitric acid, again, the hot metal is negative.

1941. If, after I show that heat applied to metals in acids or electrolytes which *can act on them* produces considerable currents, it be then said that though the metals which are inactive in the acids produce no thermo currents, those which, like copper, silver, &c. act chemically, may; then, I say, that such would be a mere supposition, and a supposition at variance with what we know of thermo-electricity; for amongst the solid conductors, metallic or non-metallic (1867.), there are none, I believe, which are able to produce thermo currents with some of the metals, and not with others. Further, these metals, copper, silver, &c., do not always show effects which can be mistaken or pass for thermo-electric, for silver in hot dilute nitric acid is scarcely different from silver in the same acid cold (1950.); and in other cases, again, the hot metals become negative instead of positive (1953.).

Cases of one metal and one electrolyte; one junction being heated.

1942. The cases I have to adduce are far too numerous to be given in detail; I will therefore describe one or two, and sum up the rest as briefly as possible.

1943. *Iron in diluted sulphuret of potassium.*—The hot iron is well positive to the cold metal. The negative and cold wire continues quite clean, but from the hot iron a dark sulphuret separates, which becoming diffused through the solution discolours it. When the cold iron is taken out, washed and wiped, it leaves the cloth clean; but that which has been heated leaves a black sulphuret upon the cloth when similarly treated.

1944. *Copper and the sulphuretted solution.*—The hot copper is well positive to the cold on the first immersion, but the effect quickly falls, from the general causes already referred to (1918.).

1945. *Tin and solution of potassa.*—The hot tin is strongly and constantly positive to the cold.

1946. *Iron and dilute sulphuric acid* (1935.).—The hot iron was constantly positive to the cold, 60° or more. *Iron and diluted nitric acid gave* even a still more striking result.

I must now enumerate merely, not that the cases to be mentioned are less decided than those already given, but to economize time.

1947. *Dilute solution of yellow sulphuret of potassium*, consisting of one volume of the strong solution (1812.), and eighteen volumes of water.—Iron, silver, and copper, with this solution, gave good results. The hot metal was positive to the cold.

1948. *Dilute solution of caustic potassa* (1932.).—Iron, copper, tin, zinc, and cadmium gave striking results in this electrolyte. The hot metal was always positive to the cold. Lead produced the same effect, but there was a momentary jerk at the galvanometer at the instant of immersion, as if the hot lead was negative at that moment. In the case of iron it was necessary to continue the application of heat, and then the formation of oxide at it could easily be observed; the alkali gradually became turbid, for the protoxide first formed was dissolved, and becoming peroxide by degrees, was deposited, and rendered the liquid dull and yellow.

1949. *Dilute sulphuric acid* (1935.).—Iron, tin, lead, and zinc, in this electrolyte, showed the power of heat to produce a current by exalting the chemical affinity, for the hot side was in each case positive.

1950. *Dilute nitric acid* is remarkable for presenting only one case of a metal hot and cold exhibiting a striking difference, and that metal is iron. With silver, copper, and zinc, the hot side is at the first moment positive to the cold, but only in the smallest degree.

1951. *Strong nitric acid.*—Hot iron is positive to cold. Both in the hot and cold acid the iron is in its peculiar state (1844. 2001.).

1952. *Dilute muriatic acid:* 1 *volume strong muriatic acid, and* 29 *volumes water.*—This acid was as remarkable for the number of cases it supplied as the dilute nitric acid was for the contrary (1950.). Iron, copper, tin, lead, zinc, and cadmium gave active circles with it, the hot metal being positive to the cold; all the results were very striking in the strength and permanency of the electric current produced.

1953. Several cases occur in which the hot metal becomes *negative* instead of positive, as above; and the principal cause of such an effect I have already adverted to (1918.). Thus with the solution of the *sulphuret of potassium* and zinc, on the first immersion of the wires into the hot and cold solution there was a pause, i. e. the galvanometer needle did not move at once, as in the former cases; afterwards a current gradually came into existence, rising in strength until the needle was deflected 70° or 80°, the hot metal being *negative* through the electrolyte to the cold metal. *Cadmium* in the same solution gave also the first pause and then a current, the hot metal being negative; but the effect was very small. Lead, hot, was negative, producing also only a feeble current. Tin gave the same result, but the current was scarcely sensible.

1954. *In dilute sulphuric acid.*—Copper and zinc, after having produced a first positive effect at the hot metal, had that reversed, and a feeble current was produced, the hot metal being negative. Cadmium gave the same phenomena, but stronger (1918.).

1955. *In dilute nitric acid.*—Lead produced no effect at the first moment; but afterwards an electric current, gradually increasing in strength, appeared, which was able to deflect the needle 20° or more, the hot metal being negative. Cadmium gave the same results as lead. Tin gave an uncertain result: at first the hot metal appeared to be a very little negative, it then became positive, and then again the current diminished, and went down almost entirely.

1956. I cannot but view in these results of the action of heat, the strongest proofs of the dependence of the electric current in voltaic circuits on the chemical action of the substances constituting these circuits: the results perfectly accord with the known influence of heat on chemical action. On the other hand, I cannot see how the theory of contact can take cognizance of them, except by adding new assumptions to those already composing it (1874.). How, for instance, can it explain the powerful effects of iron in sulphuret of potassium, or in potassa, or in dilute nitric acid; or of tin in potassa or sulphuric acid; or of iron, copper, tin, &c. in muriatic acid; or indeed of any of the effects quoted? That they cannot be due to thermo contact has been already shown by the results with inactive metals (1931. 1941.); and to these may now be added those of the active metals, silver and copper in dilute nitric acid, for heat produces scarcely a sensible effect in these cases. It seems to me that no other cause than chemical force (a very sufficient one), remains, or is needed to account for them.

1957. If it be said that, on the theory of chemical excitement, the experiments prove either too much or not enough, that, in fact, heat ought to produce the same effect with *all* the metals that are acted on by the electrolytes used, then, I say, that that does not follow. The force and other circumstances of chemical affinity vary almost infinitely with the bodies exhibiting its action, and the added effect of heat upon the chemical affinity would, necessarily, partake of these variations. Chemical action often goes on without any current being produced; and it is well known that, in almost every voltaic circuit, the chemical force has to be considered as divided into that which is local and that which is current (1120.). Now heat frequently assists the local action much, and, sometimes, without appearing to be accompanied by any great increase in the *intensity* of chemical affinity; whilst at other times we are sure, from the chemical phenomena, that it does affect the intensity of the force. The electric current, however, is not determined by the amount of action which takes place, but by the intensity of the affinities concerned; and so cases may easily be produced, in which that metal exerting the least amount of action is nevertheless the positive metal in a voltaic circuit; as with copper in weak nitric acid associated with other copper in strong acid (1975.), or iron or silver in the same weak acid against copper in the strong acid (1996.). Many of those instances where the hot side ultimately becomes negative, as of zinc in dilute solution of sulphuret of potassium (1953.), or cadmium and lead in dilute nitric acid (1955.), are of this nature; and yet the conditions and result are in perfect agreement with the chemical theory of voltaic excitement (1918.).

1958. The distinction between currents founded upon that difference of intensity which is due to the difference in force of the chemical action which is their exciting cause, is, I think, a necessary consequence of the chemical theory, and in 1834 I adopted that opinion[1] (891. 908. 916. 988.). De la Rive in 1836 gave a still more precise enunciation of such a principle[2], by saying, that the intensity of currents is exactly proportional to the degree of affinity which reigns between the particles, the combination or separation of which produces the currents.

1959. I look upon the question of the origin of the power in the voltaic battery as abundantly decided by the experimental results not connected with the action of heat (1824, &c. 1878, &c.). I further view the results with heat as adding very strong confirmatory evidence to the chemical theory; and the numerous questions which arise as to the varied results produced, only tend to show how important the voltaic circuit is as a means of investigation into the nature and principles of chemical affinity (1967.). This truth has already been most strikingly illustrated by the researches of De la Rive made by means of the galvanometer, and the investigations of my friend Professor Daniell into the real nature of acid and other compound electrolytes[3].

Cases of two metals and one electrolyte; one junction being heated.

1960. Since heat produced such striking results with single metals, I thought it probable that it might be able to affect the mutual relation of the metals in some cases, and even invert their order: on making circuits with two metals and electrolytes, I found the following cases.

1961. In the solution of *sulphuret of potassium,* hot tin is well positive to cold silver: cold tin is very slightly positive to hot silver, and the silver then rapidly tarnishes.

1962. In the solution of *potassa,* cold tin is fairly positive to hot lead, but hot tin is much more positive to cold lead. Also cold cadmium is positive to hot lead, but hot cadmium is far more positive to cold lead. In these cases, therefore, there are great differences produced by heat, but the metals still keep their order.

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[1] Philosophical Transactions, 1834, p. 428.

[2] Annales de Chimie, 1836, lxi. p. 44. &c.

[3] Philosophical Transactions, 1839, p. 97.

1963. In *dilute sulphuric acid,* hot iron is *well positive* to cold tin, but hot tin is *still more positive* to cold iron. Hot iron is a little positive to cold lead, and hot lead is very positive to cold iron. These are cases of the actual inversion of order; and tin and lead may have their states reversed exactly in the same manner.

1964. *In dilute nitric acid,* tin and iron, and iron and lead may have their states reversed, whichever is the hot metal being rendered positive to the other. If, when the iron is to be plunged into the heated side (1930.) the acid is only moderately warm, it seems at first as if the tin would almost overpower the iron, so beautifully can the forces be either balanced or rendered predominant on either side at pleasure. Lead is positive to tin in both cases; but far more so when hot than when cold.

1965. These effects show beautifully that, in many cases, when two different metals are taken, either can be made positive to the other at pleasure, by acting on their chemical affinities; though the contacts of the metals with each other (supposed to be an electromotive cause,) remain *entirely unchanged.* They show the effect of heat in reversing or strengthening the natural differences of the metals, according as its action is made to oppose or combine with their natural chemical forces, and thus add further confirmation to the mass of evidence already adduced.

1966. There are here, as in the cases of one metal, some instances where the heat renders the metal more negative than it would be if cold. They occur, principally, in the solution of sulphuret of potassium. Thus, with zinc and cadmium, or zinc and tin, the coldest metal is positive. With lead and tin, the hot tin is a little positive, cold tin very positive. With lead and zinc, hot zinc is a little positive, cold zinc much more so. With silver and lead, the hot silver is a little positive to the lead, the cold silver is more, and well positive. In these cases the current is preceded by a moment of quiescence (1953.), during which the chemical action at the hot metal reduces the efficacy of the electrolyte against it more than at the cold metal, and the latter afterwards shows its advantage.

1967. Before concluding these observations on the effects of heat, and in reference to the probable utility of the voltaic circuit in investigations of the intimate nature of chemical affinity (1959.), I will describe a result which, if confirmed, may lead to very important investigations. Tin and lead were conjoined and plunged into cold dilute sulphuric acid; the tin was positive a little. The same acid was heated, and the tin and lead, having been perfectly cleaned, were reintroduced, then the lead was a little positive to the tin. So that a difference of temperature not limited to one contact, for the two electrolytic contacts were always at the same temperature, caused a difference in the relation of these metals the one to the other. Tin and iron in dilute sulphuric acid appeared to give a similar result; i. e. in the cold acid the tin was always positive, but with hot acid the iron was sometimes positive. The effects were but small, and I had not time to enter further into the investigation.

1968. I trust it is understood that, in every case, the precautions as to very careful cleansing of the wires, the places of the ends, simultaneous immersion, observation of the first effects, &c., were attended to.

¶ v. *The exciting chemical force affected by dilution.*

1969. Another mode of affecting the chemical affinity of these elements of voltaic circuits, the metals and acids, and also applicable to the cases of such circuits, is to vary the proportion of water present. Such variation is known, by the simplest chemical experiments, to affect very importantly the resulting action, and, upon the chemical theory, it was natural to expect that it would also produce some corresponding change in the voltaic pile. The effects observed by Avogadro and Œrsted in 1823 are in accordance with such an expectation, for they found that when the same pair of metals was plunged in succession into a strong and a dilute acid, in certain cases an inversion of the current took place[1]. In 1828 De la Rive carried these and similar cases much further, especially in voltaic combinations of copper and iron with lead[1]. In 1827 Becquerel[2] experimented with one metal, copper, plunged at its two extremities into a solution of the same substance (salt) of *different strengths*; and in 1828 De la Rive[3] made many such experiments with one metal and a fluid in different states of dilution, which I think of very great importance.

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[1] Annales de Chimie, 1823, xxii. p. 361.

1970. The argument derivable from effects of this kind appeared to me so strong that I worked out the facts to some extent, and think the general results well worthy of statement. Dilution is the circumstance which most generally exalts the existing action, but how such a circumstance should increase the electromotive force of *mere contact* did not seem evident to me, without *assuming*, as before (1874.), exactly those influences at the points of contact in the various cases, which the prior results, ascertained by experiments, would require.

1971. The form of apparatus used was the bent tube already described (1915.) fig. 7. The precautions before directed with the wires, tube, &c., were here likewise needful. But there were others also requisite, consequent upon the current produced by combination of water with acid, an effect which has been described long since by Becquerel[4], but whose influence in the present researches requires explanation.

1972. Figs. 9 and 10 represent the two arrangements of fluids used, the part below *m* in the tubes being strong acid, and that above diluted. If the fluid was nitric acid and the platinum wires as in the figures, drawing the end of the wire D upwards above *m*, or depressing it from above *m* downwards, caused great changes at the galvanometer; but if they were preserved quiet at any place, then the electro-current ceased, or very nearly so. Whenever the current existed it was from the weak to the strong acid through the liquid.

1973. When the tube was arranged, as in fig. 9, with water or dilute acid on one side only, and the wires were immersed not more than one third of an inch, the effects were greatly diminished; and more especially, if, by a little motion with a platinum wire, the acids had been mixed at *m*, so that the transition from weak to strong was gradual instead of sudden. In such cases, even when the wires were moved, horizontally, in the acid, the effect was so small as to be scarcely sensible, and not likely to be confounded with the chemical effects to be described hereafter. Still more surely to avoid such interference, an acid moderately diluted was used instead of water. The precaution was taken of emptying, washing, and re-arranging the tubes with fresh acid after each experiment, lest any of the metal dissolved in one experiment should interfere with the results of the next.

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[1] Annales de Chimie, 1828, xxxvii. p. 234.

[2] Ibid. 1827, xxxv. p. 120.

[3] Ibid. 1828, xxxvii. p. 240, 241.

[4] Traité de l'Electricité, ii. p. 81.

1974. I occasionally used the tube with dilute acid on one side only, fig. 9, and sometimes that with dilute acid on both sides, fig. 10. I will call the first No. 1. and the second No. 2.

1975. In illustration of the general results I will describe a particular case. Employing tube No. 1. with strong and dilute nitric acid[1], and two copper wires, the wire in the dilute acid was powerfully positive to the one in the strong acid at the first moment, and continued so. By using tube No. 2. the galvanometer-needle could be held stiffly in either direction, simply by simultaneously raising one wire and depressing the other, so that the first should be in weak and the second in strong acid; the former was always the positive piece of metal.

1976. On repeating the experiments with the substitution of platinum, gold, or even palladium for the copper, scarcely a sensible effect was produced (1973.).

1977. *Strong and dilute nitric acid*[1].—The following single metals being compared with themselves in these acids, gave most powerful results of the kind just described with copper (1975.); silver, iron, lead, tin, cadmium, zinc. The metal in the weaker acid was positive to that in the stronger. Silver is very changeable, and after some time the current is often suddenly reversed, the metal in the strong acid becoming positive: this again will change back, the metal in the weaker acid returning to its positive state. With tin, cadmium, and zinc, violent action in the acid quickly supervenes and mixes all up together. Iron and lead show the alternations of state in the tube No. 2. as beautifully as copper (1975.).

1978. *Strong and dilute sulphuric acid.*—I prepared an acid of 49 by weight, strong oil of vitriol, and 9 of water, giving a sulphuric acid with two proportions of water, and arranged the tube No. 1. (1974.) with this and the strongest acid. But as this degree of dilution produced very little effect with the iron, as compared with what a much greater dilution effected, I adopted the plan of putting strong acid into the tube, and then adding a little water at the top at one of the sides, with the precaution of stirring and cooling it previous to the experiment (1973.).

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[1] The dilute acid consisted of three volumes of strong nitric acid and two volumes of water.

1979. With *iron*, the part of the metal in the weaker acid was powerfully positive to that in the stronger acid. With copper, the same result, as to direction of the current, was produced; but the amount of the effect was small. With silver, cadmium, and zinc, the difference was either very small or unsteady, or nothing; so that, in comparison with the former cases, the electromotive action of the strong and weak acid appeared balanced. With lead and tin, the part of the metal in the *strong* acid was *positive* to that in the weak acid; so that they present an effect the reverse of that produced by iron or copper.

1980. *Strong and dilute muriatic acid.*—I used the strongest pure muriatic acid in tube No. 1, and added water on the top of one side for the dilute extremity (1973.), stirring it a little as before. With silver, copper, lead, tin, cadmium, and zinc, the metal in the *strongest acid* was positive, and the current in most cases powerful. With iron, the end in the strongest acid was first positive: but shortly after the weak acid side became positive and continued so. With palladium, gold, and platinum, nearly insensible effects were the results.

1981. *Strong and dilute solution of caustic potassa.*—With iron, copper, lead, tin, cadmium, and zinc, the metal in the strong solution was positive: in the case of iron slightly, in the case of copper more powerfully, deflecting the needle 30° or 38°, and in the cases of the other metals very strongly. Silver, palladium, gold, and platinum, gave the merest indications (1973.).

Thus potash and muriatic acid are, in several respects, contrasted with nitric and sulphuric acids. As respects muriatic acid, however, and perhaps even the potash, it may be admitted that, even in their strongest states, they are not fairly comparable to the very strong nitric and sulphuric acids, but rather to those acids when somewhat diluted (1985.).

1982. I know it may be said in reference to the numerous changes with strong and dilute acids, that the results are the consequence of corresponding alterations in the contact force; but this is to change about the theory with the phenomena and with chemical force (1874. 1956. 1985. 2006. 2014. 2063.); or it may be alleged that it is the contact force of the solutions produced at the metallic surfaces which, differing, causes difference of effect; but this is to put the effect before the cause in the order of *time*. If the liberty of shifting the point of efficacy from metals to fluids, or from one place to another, be claimed, it is at all events quite time that some definite statement and data respecting the active points (1808.) should be given. At present it is difficult to lay hold of the contact theory by any argument derived from experiment, because of these uncertainties or variations, and it is in that respect in singular contrast with the definite expression as to the place of action which the chemical theory supplies.

1983. All the variations which have been given are consistent with the extreme variety which chemical action under different circumstances possesses, but, as it still appears to me, are utterly incompatible with, what should be, the simplicity of mere contact action; further they admit of even greater variation, which renders the reasons for the one view and against the other, still more conclusive.

1984. Thus if a contact philosopher say that it is only the very strongest acids that can render the part of the metals in it negative, and therefore the effect does not happen with muriatic acid or potash (1980. 1981.), though it does with nitric and sulphuric acids (1977. 1978.); then, the following result is an answer to such an assumption. Iron in *dilute nitric acid*, consisting of one volume of strong acid and twenty of water, is positive to iron in strong acid, or in a mixture of one volume of strong acid with one of water, or with three, or even with five volumes of water. Silver also, in the weakest of these acids, is positive to silver in any of the other four states of it.

1985. Or if, modifying the statement upon these results, it should be said that diluting the acid at one contact *always* tends to give it a certain *proportionate* electromotive force, and therefore diluting one side more than the other will still allow this force to come into play; then, how is it that with muriatic acid and potassa the effect of dilution is the reverse of that which has been quoted in the cases with nitric acid and iron or silver? (1977. 1984.) Or if, to avoid *difficulty*, it be assumed that each electrolyte must be considered apart, the nitric acid by itself, and the muriatic acid by itself, for that one may differ from another in the *direction* of the change induced by dilution, then how can the following results with a single acid be accounted for?

1986. I prepared four nitric acids:

A was very strong pure nitric acid;
B was one volume of A and one volume of water;
C was one volume of A and three volumes of water;
D was one volume of A and twenty volumes of water.

Experimenting with these acids and a metal, I found that copper in C acid was positive to copper in A or D acid. Nor was it the *first* addition of water to the strong acid that brought about this curious relation, for copper in the B acid was positive to copper in the strong acid A, but negative to the copper in the weak acid D: the negative effect of the stronger nitric acid with this metal does not therefore depend upon a very high degree of concentration.

1987. Lead presents the same beautiful phenomena. In the C acid it is positive to lead either in A or D acid: in B acid it is positive to lead in the strongest, and negative to lead in the weakest acid.

1988. I prepared also three sulphuric acids:

E was strong oil of vitriol;
F one volume of E and two volumes of water;
G one volume of E and twenty volumes of water.

Lead in F was well *negative* to lead either in E or G. Copper in F was also negative to copper in E or G, but in a smaller degree. So here are two cases in which metals in an acid of a certain strength are *negative* to the same metals in the same acid, either stronger or weaker. I used platinum wires ultimately in all these cases with the same acids to check the interference of the combination of acid and water (1973.); but the results were then almost nothing, and showed that the phenomena could not be so accounted for.

1989. To render this complexity for the contact theory still more complicated, we have further variations, in which, with the same acid strong and diluted, some metals are positive in the strong acid and others in the weak. Thus, tin in the strongest sulphuric acid E (1988.) was positive to tin in the moderate or the weak acids F and G; and tin in the moderate acid F was positive to the same metal in G. Iron, on the contrary, being in the strong acid E was negative to the weaker acids F and G; and iron in the medium acid F was negative to the same metal in G.

1990. For the purpose of understanding more distinctly what the contact theory has to do here, I will illustrate the case by a diagram. Let fig. 11 represent a circle of metal and sulphuric acid. If A be an arc of iron or copper, and B C strong oil of vitriol, there will be no determinate current: or if B C be weak acid, there will be no such current: but let it be strong acid at B, and diluted at C, and an electric current will run round A C B. If the metal A be silver, it is equally indifferent with the strong and also with the weak acid, as iron has been found to be as to the production of a current; but, besides that, it is indifferent with the strong acid at B and the weak acid at C. Now if the dilution of the electrolyte at one part, as C, had so far increased the contact electromotive force there, when iron or copper was present, as to produce the current found by experiment; surely it ought (consistently with any reasonable limitations of the assumptions in the contact theory,) to have produced the same effect with silver: but there was none. Making the metal A lead or tin, the difficulty becomes far greater; for though with the strong or the weak acid alone any effect of a determinate current is nothing, yet one occurs upon dilution at C, but now dilution must be supposed to *weaken* instead of *strengthen* the contact force, for the current is in the reverse direction.

1991. Neither can these successive changes be referred to a gradual progression in the effect of dilution, dependent upon the *order of the metals.* For supposing dilution more favourable to the electromotive force of the contact of an acid and a metal, *in proportion* as the metals were in a certain order, as for instance that of their efficacy in the voltaic battery; though such an assumption might seem to account for the gradual diminution of effect from iron to copper, and from copper to silver, one would not expect the reverse effects, or those on the other side of zero, to appear by a return back to such metals as lead and tin (1979. 1989.), but rather look for them in platinum or gold, which, however, produce no results of the kind (1976. 1988.). To increase still further this complexity, it appears, from what has been before stated, that on changing the *acids* the order must again be changed (1981.); nay, more, that with the same acid, and merely by changing the proportion of dilution, such alteration of the order must take place (1986. 1988.).

1992. Thus it appears, as before remarked (1982.), that to apply the theory of contact electromotive force to the facts, that theory must twist and bend about with every variation of chemical action; and after all, with every variety of contact, active and inactive, in no case presents phenomena independent of the active exertion of chemical force.

1993. As the influence of dilution and concentration was so strong in affecting the relation of different parts of the same metal to an acid, making one part either positive or negative to another, I thought it probable that, by mere variation in the strength of the interposed electrolyte, the order of metals when in acids or other solutions of uniform strength, might be changed. I therefore proceeded to experiment on that point, by combining together two metals, tin and lead, through the galvanometer (1915.); arranging the electrolytic solution in tube No. 1, strong on one side and weak on the other: immersing the wires simultaneously, tin into the strong, and lead into the weak solution, and after observing the effect, re-cleaning the wires, re-arranging the fluid, and re-immersing the wires, the tin into the weak, and the lead into the strong portion. De la Rive has already stated[1] that inversions take place when dilute and strong sulphuric acid is used; these I could not obtain when care was taken to avoid the effect of the investing fluid (1918.): the general statement is correct, however, when applied to another acid, and I think the evidence very important to the consideration of the great question of contact or chemical action.

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[1] Annales de Chimie, 1828, xxxvii. p. 240.

1994. *Two metals in strong and weak solution of potash.*—Zinc was positive to tin, cadmium, or lead, whether in the weak or strong solution. Tin was positive to cadmium, either in weak or strong alkali. Cadmium was positive to lead both ways, but most when in the strong alkali. Thus, though there were *differences in degree* dependent on the strength of the solution, there was *no inversion* of the order of the metals.

1995. *Two metals in strong and weak sulphuric acid.*—Cadmium was positive to iron and tin both ways: tin was also positive to iron, copper, and silver; and iron was positive to copper and silver, whichever side the respective metals were in. Thus none of the metals tried could be made to pass the others, and so take a different order from that which they have in acid uniform in strength. Still there were great variations in degree; thus iron in strong acid was only a little positive to silver in weak acid, but iron in weak acid was very positive to silver in strong acid. Generally the metal, usually called positive, was most positive in the weak acid; but that was not the case with lead, tin, and zinc.

1996. *Two metals in strong and weak nitric acid.*—Here the degree of change produced by difference in the strength of the acid was so great, as to cause not merely difference in degree, but inversions of the order of the metals, of the most striking nature. Thus iron and silver being in tube No. 2 (1974.), whichever metal was in the weak acid was positive to the other in the strong acid. It was merely requisite to raise the one and lower the other metal to make either positive at pleasure (1975.). Copper in weak acid was positive to silver, lead, or tin, in strong acid. Iron in weak acid was positive to silver, copper, lead, zinc, or tin, in strong acid. Lead in weak acid was positive to copper, silver, tin, cadmium, zinc, and iron in strong acid. Silver in weak acid was positive to iron, lead, copper, and, though slightly, even to tin, in strong acid. Tin in weak acid was positive to copper, lead, iron, zinc, and silver, and either neutral or a little positive to cadmium in strong acid. Cadmium in weak acid is very positive, as might be expected, to silver, copper, lead, iron, and tin, and, moderately so, to zinc in the strong acid. When cadmium is in the strong acid it is slightly positive to silver, copper, and iron, in weak acid. Zinc in weak acid is very positive to silver, copper, lead, iron, tin, and cadmium in strong acid: when in the strong acid it is a little positive to silver and copper in weak acid.

1997. Thus wonderful changes occur amongst the metals in circuits containing this acid, merely by the effect of dilution; so that of the five metals, silver, copper, iron, lead, and tin, any one of them can be made either positive or negative to any other, with the exception of silver positive to copper. The order of these five metals only may therefore be varied about one hundred different ways in the same acid, merely by the effect of dilution.

1998. So also zinc, tin, cadmium, and lead; and likewise zinc, tin, iron, and lead, being groups each of four metals; any one of these metals may be made either positive or negative to any other metal of the same group, by dilution of this acid.

1999. But the case of variation by dilution may, as regards the opposed theories, be made even still stronger than any yet stated; for the *same metals* in the *same acid* of the *same strength at the two sides* may be made to change their order, as the chemical action of the acid on each particular metal is affected, by dilution, in a smaller or greater degree.

2000. A voltaic association of iron and silver was dipped, both metals at once, into the same strong nitric acid; for the first instant, the iron was positive; the moment after, the silver became positive, and continued so. A similar association of iron and silver was put into weak nitric acid, and the iron was immediately positive, and continued so. With iron and copper the same results were obtained.

2001. These, therefore, are *finally* cases of such an inversion (1999.); but as the iron in the strong nitric acid acquires a state the moment after its immersion, which is probably not assumed by it in the weak acid (1843. 1951. 2033.), and as the action on the iron in its *ordinary* state may be said to be, to render it positive to the silver or copper, both in the strong or weak acid, we will not endeavour to force the fact, but look to other metals.

2002. *Silver and nickel* being associated in weak nitric acid, the nickel was positive; being associated in strong nitric acid, the nickel was still positive at the first moment, but the silver was finally positive. The nickel lost its superiority through the influence of an investing film (1918.); and though the effect might easily pass unobserved, the case cannot be allowed to stand, as fulfilling the statement made (1999.).

2003. *Copper and nickel* were put into strong nitric acid; the copper was positive from the first moment. Copper and nickel being in dilute nitric acid, the nickel was slightly but clearly positive to the copper. Again, *zinc and cadmium* in strong nitric acid; the cadmium was positive strongly to the zinc; the same metals being in dilute nitric acid, the zinc was very positive to the cadmium. These I consider beautiful and unexceptionable cases (1999.).

2004. Thus the nitric acid furnishes a most wonderful variety of effects when used as the electrolytic conductor in voltaic circles; and its difference from sulphuric acid (1995.) or from potassa (1994.) in the phenomena consequent upon dilution, tend, in conjunction with many preceding facts and arguments, to show that the electromotive force in a circle is not the consequence of any power in bodies generally, belonging to them in classes rather than as individuals, and having that simplicity of character which contact force has been assumed to have; but one that has all the variations which chemical force is *known* to exhibit.

2005. The changes occurring where any one of four or five metals, differing from each other as far as silver and tin, can be made positive or negative to the others (1997. 1998.), appears to me to shut out the probability that the contact of these metals with each other can produce the smallest portion of the effect in these voltaic arrangements; and then, if not there, neither can they be effective in any other arrangements; so that what has been deduced in that respect from former experiments (1829. 1833.) is confirmed by the present.

2006. Or if the scene be shifted, and it be said that it is the *contact* of the acids or solutions which, by dilution at one side, produce these varied changes (1874. 1982. 1991. 2014. 2060.), then how *utterly unlike* such contact must be to that of the numerous class of conducting solid bodies (1809. 1867.)! and where, to give the assumption any show of support, is the case of such contact (apart from chemical action) producing such currents?

2007. That it cannot be an alteration of contact force by mere dilution at one side (2006.) is also shown by making such a change, but using metals that are chemically inactive in the electrolyte employed. Thus when nitric or sulphuric acids were diluted at one side, and then the strong and the weak parts connected by platinum or gold (1976.), there was no sensible current, or only one so small as to be unimportant.

2008. A still stronger proof is afforded by the following result. I arranged the tube, fig. 9 (1972.), with strong solution of yellow sulphuret of potassium (1812.) from A to *m*, and a solution consisting of one volume of the strong solution, with six of water from *m* to B. The extremities were then connected by platinum and iron in various ways; and when the first effect of immersion was guarded against, including the first brief negative state of the iron (2049.), the effects were as follows. Platinum being in A and in B, that in A, or the strong solution, was very slightly positive, causing a permanent deflection of 2°. Iron being in A and in B, the same result was obtained. Iron being in A and platinum in B, the iron was positive about 2° to the platinum. Platinum being in A and iron in B, the platinum was now positive to the iron by about 2°. So that not only the contact of the iron and platinum passes for nothing, but the contact of strong and weak solution of this electrolyte with either iron or platinum, is ineffectual in producing a current. The current which is constant is very feeble, and evidently related to the mutual position of the strong and weak solutions, and is probably due to their gradual mixture.

2009. The results obtained by dilution of an electrolyte capable of acting on the metals employed to form with it a voltaic circuit, may in some cases depend on making the acid a better electrolyte. It would appear, and would be expected from the chemical theory, that whatever circumstance tends to make the fluid a more powerful chemical agent and a better electrolyte, (the latter being a relation purely chemical and not one of contact,) favours the production of a determinate current. Whatever the cause of the effect of dilution may be, the results still tend to show how valuable the voltaic circle will become as an investigator of the nature of chemical affinity (1959.).

¶ vi. *Differences in the order of the metallic elements of voltaic circles.*

2010. Another class of experimental arguments, bearing upon the great question of the origin of force in the voltaic battery, is supplied by a consideration of the different order in which the metals appear as electromotors when associated with different exciting electrolytes. The metals are usually arranged in a certain order; and it has been the habit to say, that a metal in the list so arranged is negative to any one above it, and positive to any one beneath it, as if (and indeed upon the conviction that) they possessed a certain direct power one with another. But in 1812 Davy showed inversions of this order in the case of iron and copper[1] (943.); and in 1828 De la Rive showed many inversions in different cases[2] (1877.); gave a strong contrast in the order of certain metals in strong and dilute nitric acid[3]; and in objecting to Marianini's result most clearly says, that any order must be considered in relation only to that liquid employed in the experiments from which the order is derived[4].

2011. I have pursued this subject in relation to several solutions, taking the precautions before referred to (1917, &c.), and find that no such single order as that just referred to can be maintained. Thus nickel is negative to antimony and bismuth in strong nitric acid; it is positive to antimony and bismuth in dilute nitric acid; it is positive to antimony and negative to bismuth in strong muriatic acid; it is positive to antimony and bismuth in dilute sulphuric acid; it is negative to bismuth and antimony in potash; and it is very negative to bismuth and antimony, either in the colourless or the yellow solution of sulphuret of potassium.

2012. In further illustration of this subject I will take ten metals, and give their order in seven different solutions.

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[1] Elements of Chemical Philosophy, p. 149.

[2] Annales de Chimie, 1828, xxxvii. p. 232.

[3] Ibid., p. 235.

Ibid., p. 243.

Dilute nitric acid.	Dilute sulphuric acid.	Muriatic acid.	Strong nitric acid.	Solution of caustic potassa.	Colourless bihydrosulphuret of potassium.	Yellow hydrosulphuret of potassium.
1. Silver.	1. Silver.	3. Antimony.	5. Nickel.	1. Silver.	6. Iron.	6. Iron.
2. Copper.	2. Copper.	1. Silver.	1. Silver.	5. Nickel.	5. Nickel.	5. Nickel.
3. Antimony.	3. Antimony.	5. Nickel.	3. Antimony.	2. Copper.	4. Bismuth.	4. Bismuth.
4. Bismuth.	4. Bismuth.	4. Bismuth.	2. Copper.	6. Iron.	8. Lead.	3. Antimony.
5. Nickel.	5. Nickel.	2. Copper.	4. Bismuth.	4. Bismuth.	1. Silver.	8. Lead.
6. Iron.	6. Iron.	6. Iron.	6. Iron.	8. Lead.	3. Antimony.	1. Silver.
7. Tin.	8. Lead.	8. Lead.	7. Tin.	3. Antimony.	7. Tin.	7. Tin.
8. Lead.	7. Tin.	7. Tin.	8. Lead.	9. Cadmium.	2. Copper.	9. Cadmium.
9. Cadmium.	9. Cadmium.	9. Cadmium.	10. Zinc.	7. Tin.	10. Zinc.	2. Copper.
10. Zinc.	10. Zinc.	10. Zinc.	9. Cadmium.	10. Zinc.	9. Cadmium.	10. Zinc.

2013. The dilute nitric acid consisted of one volume strong acid and seven volumes of water; the dilute sulphuric acid, of one volume strong acid and thirteen of water; the muriatic acid, of one volume strong solution and one volume water. The strong nitric acid was pure, and of specific gravity 1·48. Both strong and weak solution of potassa gave the same order. The yellow sulphuret of potassium consisted of one volume of strong solution (1812.) and five volumes of water. The metals are numbered in the order which they presented in the dilute acids (the negative above), for the purpose of showing, by the comparison of these numbers in the other columns, the striking departures there, from this, the most generally assumed order. Iron is included, but only in its ordinary state: its place in nitric acid being given as that which it possesses on its first immersion, not that which it afterwards acquires.

2014. The displacements appear to be most extraordinary, as extraordinary as those consequent on dilution (2005.); and thus show that there is no general ruling influence of fluid conductors, or even of acids, alkalies, &c. as distinct classes of such conductors, apart from their pure chemical relations. But how can the contact theory account for these results? To meet such facts it must be bent about in the most extraordinary manner, following all the contortions of the string of facts (1874. 1956. 1992. 2006. 2063.), and yet never showing a case of the production of a current by contact alone, i. e. unaccompanied by chemical action.

2015. On the other hand, how simply does the chemical theory of excitement of the current represent the facts! as far as we can yet follow them they go hand in hand. Without chemical action, no current; with the changes of chemical action, changes of current; whilst the influence of the strongest cases of *contact*, as of silver and tin (1997.) with each other, pass for nothing in the result. In further confirmation, the exciting power does not rise, but fall, by the contact of the bodies produced, as the chemical actions producing these decay or are exhausted; the consequent result being well seen in the effect of the investing fluids produced (1918. 1953. 1966.).

2016. Thus, as De la Rive has said, any list of metals in their order should be constructed in reference to the exciting fluid selected. Further, a zero point should be expressed in the series; for as the electromotive power may be either at the anode or cathode (2040. 2052.), or jointly at both, that substance (if there be one) which is absolutely without any exciting action should form the zero point. The following may be given, by way of illustration, as the order of a few metals, and other substances in relation to muriatic acid:

Peroxide of lead,
Peroxide of manganese,
Oxide of iron,
PLUMBAGO,
Rhodium,
Platinum,
Gold,
Antimony,
Silver,
Copper,
Zinc:

in which plumbago is the neutral substance; those in italics are active at the cathode, and those in Roman characters at the anode. The upper are of course negative to the lower. To make such lists as complete as they will shortly require to be, numbers expressive of the relative exciting force, counting from the zero point, should be attached to each substance.

¶ vii. *Active voltaic circles and batteries without metallic contact.*

2017. There are cases in abundance of electric currents produced by pure chemical action, but not one undoubted instance of the production of a current by pure contact. As I conceive the great question must now be settled by the weight of evidence, rather than by simple philosophic conclusions (1799.), I propose adding a few observations and facts to show the number of these cases, and their force. In the Eighth Series of these Researches[1] (April, 1834) I gave the first experiment, that I am aware of, in which chemical action was made to produce an electric current and chemical decomposition at a distance, in a simple circuit, without any contact of metals (880, &c.). It was further shown, that when a pair of zinc and platinum plates were excited at one end of the dilute nitro-sulphuric acid (880.), or solution of potash (884.), or even in some cases a solution of common salt (885.), decompositions might be produced at the other end, of solutions of iodide of potassium (900.), protochloride of tin (901.), sulphate of soda, muriatic acid, and nitrate of silver (906.); or of the following bodies in a state of fusion; nitre, chlorides of silver and lead, and iodide of lead (902. 906.); no metallic contact being allowed in any of the experiments.

2018. I will proceed to mention new cases; and first, those already referred to, where the action of a little dilute acid produced a current passing through the solution of the sulphuret of potassium (1831.), or green nitrous acid (1844.), or the solution of potassa (1854.); for here no metallic contact was allowed, and chemical action was the evident and only cause of the currents produced.

2019. The following is a table of cases of similar excitement and voltaic action, produced by chemical action without metallic contact. Each horizontal line contains the four substances forming a circuit, and they are so arranged as to give the direction of the current, which was in all cases from left to right through the bodies as they now stand. All the combinations set down were able to effect decomposition, and they are but a few of those which occurred in the course of the investigation.

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[1] Philosophical Transactions, 1834, p. 426.

2020.

Iron.	Dilute nitric acid.	Platinum.	Sulph. of Potassium (1812.)	Full current.
Iron.	Dilute nitric acid.	Platinum.	Red nitric acid.	Full current.
Iron.	Dilute nitric acid.	Platinum.	Pale nitric acid, strong.	Good.
Iron.	Dilute nitric acid.	Platinum.	Green nitrous acid.	Very powerful.
Iron.	Dilute nitric acid.	Platinum.	Iodide of potassium.	Full current.
Iron.	Dilute sulphuric acid.	Platinum.	Sulphuret of potassium.	Full.
Iron.	Dilute sulphuric acid.	Platinum.	Red nitric acid.	Good.
Iron.	Muriatic acid.	Platinum.	Green nitrous acid.	Most powerful.
Iron.	Dilute muriatic acid.	Platinum.	Red nitric acid.	Good.
Iron.	Dilute muriatic acid.	Platinum.	Sulphuret of potassium.	Good.
Iron.	Solution of salt.	Platinum.	Green nitrous acid.	Most powerful.
Iron.	Common water.	Platinum.	Green nitrous acid.	Good.
Zinc.	Dilute nitric acid.	Platinum.	Iodide of potassium.	Good.
Zinc.	Muriatic acid.	Platinum.	Iodide of potassium.	Good.
Cadmium.	Dilute nitric acid.	Platinum.	Iodide of potassium.	Good.
Cadmium.	Muriatic acid.	Platinum.	Iodide of potassium.	Good.
Lead.	Dilute nitric acid.	Platinum.	Iodide of potassium.	Good.
Lead.	Muriatic acid.	Platinum.	Iodide of potassium.	Good.
Copper.	Dilute nitric acid.	Platinum.	Iodide of potassium.	
Copper.	Muriatic acid.	Platinum.	Iodide of potassium.	
Lead.	Strong sulphuric acid.	Iron.	Dilute sulphuric acid.	Strong.
Tin.	Strong sulphuric acid.	Iron.	Dilute sulphuric acid.	Strong.
Copper.	Sulphuret of potassium.	Iron.	Dilute nitric acid.	Powerful.
Copper.	Sulphuret of potassium.	Iron.	Iodide of potassium.	
Copper.	Strong nitric acid.	Iron.	Dilute nitric acid.	Very powerful.
Copper.	Strong nitric acid.	Iron.	Iodide of potassium.	
Silver.	Strong nitric acid.	Iron.	Dilute nitric acid.	Strong.
Silver.	Strong nitric acid.	Iron.	Iodide of potassium.	Good.
Silver.	Sulphuret of potassium.	Iron.	Dilute nitric acid.	Strong.
Tin.	Strong sulphuric acid.	Copper.	Dilute sulphuric acid.	

2021. It appears to me probable that any one of the very numerous combinations which can be made out of the following Table, by taking one substance from each column and arranging them in the order in which the columns stand, would produce a current without metallic contact, and that some of these currents would be very powerful.

Rhodium	Strong nitrous acid, or strong solution of sulphuret of potassium.	Iron	Dilute nitric acid
Gold			Dilute sulphuric acid
Platinum			Muriatic acid
Palladium			Solution of vegetable acids
Silver			Iodide of potassium
Nickel			Iodide of zinc
Copper			Solution of salt
Lead			Many metallic solutions.
Tin			
Zinc			
Cadmium			

2022. To these cases must be added the many in which one metal in a uniform acid gave currents when one side was heated (1942, &c.). Also those in which one metal with an acid strong and diluted gave a current (1977, &c.).

2023. In the cases where by dilution of the acid one metal can be made either positive or negative to another (1996, &c.), one half of the results should be added to the above, except that they are too strong; for instead of proving that chemical action can produce a current without contact, they go to the extent of showing a total disregard of it, and production of the current against the force of contact, as easily as with it.

2024. That it is easy to construct batteries without metallic contact was shown by Sir Humphry Davy in 1801[1], when he described various effective arrangements including only one metal. At a later period Zamboni constructed a pile in which but one metal and one fluid was used[2], the only difference being extent of contact at the two surfaces. The following forms, which are dependent upon the mere effect of dilution, may be added to these.

2025. Let *a b*, *a b*, *a b*, fig. 12, Plate III., represent tubes or other vessels, the parts at *a* containing strong nitric or sulphuric acid, and the parts at *b* dilute acid of the same kind; then connect these by wires, rods, or plates of one metal only, being copper, iron, silver, tin, lead, or any of those metals which become positive and negative by difference of dilution in the acid (1979, &c.). Such an arrangement will give an effective battery.

2026. If the acid used be the sulphuric, and the metal employed be iron, the current produced will be in one direction, thus ⟵, through the part figured; but if the metal be tin, the resulting current will be in the contrary direction, thus ⟶.

2027. Strong and weak solutions of potassa being employed in the tubes, then the single metals zinc, lead, copper, tin, and cadmium (1981.), will produce a similar battery.

2028. If the arrangements be as in fig. 13, in which the vessels 1, 3, 5, &c. contain strong sulphuric acid, and the vessels 2, 4, 6, &c. dilute sulphuric acid; and if the metals *a*, *a*, *a*, are tin, and *b*, *b*, *b*, are iron (1979.), a battery electric current will be produced in the direction of the arrow. If the metals be changed for each other, the acids remaining; or the acids be changed, the metals remaining; the direction of the current will be reversed.

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[1] Philosophical Transactions, 1801, p. 397. Also Journals of the Royal Institution, 1802, p. 51; and Nicholson's Journal, 8vo, 1802, vol. i. p. 144.

[2] Quarterly Journal of Science, viii. 177; or Annales de Chimie, xi. 190. (1819.).

¶ viii. *Considerations of the sufficiency of chemical action.*

2029. Thus there is no want of cases in which chemical action alone produces voltaic currents (2017.); and if we proceed to look more closely to the correspondence which ought to exist between the chemical action and the current produced, we find that the further we trace it the more exact it becomes; in illustration of which the following cases will suffice.

2030. *Chemical action does evolve electricity.*—This has been abundantly proved by Becquerel and De la Rive. Becquerel's beautiful voltaic arrangement of acid and alkali[1] is a most satisfatory proof that chemical action is abundantly sufficient to produce electric phenomena. A great number of the results described in the present papers prove the same statement.

2031. *Where chemical action has been, but diminishes or ceases, the electric current diminishes or ceases also.*—The cases of tin (1882. 1884.), lead (1885.), bismuth (1895.), and cadmium (1905.), in the solution of sulphuret of potassium, are excellent instances of the truth of this proposition.

2032. If a piece of grain tin be put into strong nitric acid, it will generally exert no action, in consequence of the film of oxide which is formed upon it by the heat employed in the process of breaking it up. Then two platinum wires, connected by a galvanometer, may be put into the acid, and one of them pressed against the piece of tin, yet without producing an electric current. If, whilst matters are in this position, the tin be scraped under the acid by a glass rod, or other non-conducting substance capable of breaking the surface, the acid acts on the metal newly exposed, and produces a current; but the action ceases in a moment or two from the formation of oxide of tin and an exhausted investing solution (1918.), and the current ceases with it. Each scratch upon the surface of the tin reproduces the series of phenomena.

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[1] Annales de Chimie, 1827, xxxv. p. 122. Bibliothèque Universelle, 1838; xiv. 129, 171.

2033. The case of iron in strong nitric acid, which acts and produces a current at the first moment (1843. 1951. 2001.), but is by that action deprived of so much of its activity, both chemical and electrical, is also a case in point.

2034. If lead and tin be associated in muriatic acid, the lead is positive at the first moment to the tin. The tin then becomes positive, and continues so. This change I attribute to the circumstance, that the chloride of lead formed partly invests that metal, and prevents the continuance of the action there; but the chloride of tin, being far more soluble than that of lead, passes more readily into the solution; so that action goes on there, and the metal exhibits a permanent positive state.

2035. The effect of the investing fluid already referred to in the cases of tin (1919.) and cadmium (1918.), some of the results with two metals in hot and cold acid (1966.), and those cases where metal in a heated acid became negative to the same metal in cold acid (1953, &c.), are of the same kind. The latter can be beautifully illustrated by two pieces of lead in dilute nitric acid: if left a short time, the needle stands nearly at 0°, but on heating either side, the metal there becomes negative 20° or more, and continues so as long as the heat is continued. On cooling that side and heating the other, that piece of lead which before was positive now becomes negative in turn, and so on for any number of times.

2036. *When the chemical action changes the current changes also.*—This is shown by the cases of two pieces of the same active metal in the same fluid. Thus if two pieces of silver be associated in strong muriatic acid, first the one will be positive and then the other; and the changes in the direction of the current will not be slow as if by a gradual action, but exceedingly sharp and sudden. So if silver and copper be associated in a dilute solution of sulphuret of potassium, the copper will be chemically active and positive, and the silver will remain clean; until of a sudden the copper will cease to act, the silver will become instantly covered with sulphuret, showing by that the commencement of chemical action there, and the needle of the galvanometer will jump through 180°. Two pieces of silver or of copper in solution of sulphuret of potassium produce the same effect.

2037. If metals be used which are inactive in the fluids employed, and the latter undergo no change during the time, from other circumstances, as heat, &c. (1838. 1937.), then no currents, and of course no such alterations in direction, are produced.

2038. *Where no chemical action occurs no current is produced.*—This in regard to ordinary solid conductors, is well known to be the case, as with metals and other bodies (1867.). It has also been shown to be true when fluid conductors (electrolytes) are used, in every case where they exert no chemical action, though such different substances as acid, alkalies and sulphurets have been employed (1843. 1853. 1825. 1829.). These are very striking facts.

2039. *But a current will occur the moment chemical action commences.*—This proposition may be well illustrated by the following experiment. Make an arrangement like that in fig. 14: the two tubes being charged with the same pure, pale, strong nitric acid, the two platinum wires *p p* being connected by a galvanometer, and the wire *i*, of iron. The apparatus is only another form of the simple arrangement fig. 15, where, in imitation of a former experiment (889.), two plates of iron and platinum are placed parallel, but separated by a drop of strong nitric acid at each extremity. Whilst in this state no current is produced in either apparatus; but if a drop of water be added at *b* fig. 15, chemical action commences, and a powerful current is produced, though without metallic or any additional contact. To observe this with the apparatus, fig. 14, a drop of water was put in at *b*. At first there was no chemical action and no electric current, though the water was there, so that contact with the water did nothing: the water and acid were moved and mixed together by means of the end of the wire *i*; in a few moments proper chemical action came on, the iron evolving nitrous gas at the place of its action, and at the same time acquiring a positive condition at that part, and producing a powerful electric current.

2040. *When the chemical action which either has or could have produced a current in one direction is reversed or undone, the current is reversed* (*or undone*) *also.*

2041. This is a principle or result which most strikingly confirms the chemical theory of voltaic excitement, and is illustrated by many important facts. Volta in the year 1802[1], showed that crystallized *oxide of manganese* was highly negative to zinc and similar metals, giving, according to his theory, electricity to the zinc at the point of contact. Becquerel worked carefully at this subject in 1835[2], and came to the conclusion, but reservedly expressed, that the facts were favourable to the theory of contact. In the following year De la Rive examined the subject[3], and shows, to my satisfaction at least, that the peroxide is at the time undergoing chemical change and losing oxygen, a change perfectly in accordance with the direction of the current it produces.

2042. The peroxide associated with platinum in the green nitrous acid originates a current, and is negative to the platinum, at the same time giving up oxygen and converting the nitrous acid into nitric acid, a change easily shown by a common chemical experiment. In nitric acid the oxide is negative to platinum, but its negative state is much increased if a little alcohol be added to the acid, that body assisting in the reduction of the oxide. When associated with platinum in solution of potash, the addition of a little alcohol singularly favours the increase of the current for the same reason. When the peroxide and platinum are associated with solution of sulphuret of potassium, the peroxide, as might have been expected, is strongly negative.

2043. In 1835 M. Muncke[4] observed the striking power of peroxide of lead to produce phenomena like those of the peroxide of manganese, and these M. de la Rive in 1836 immediately referred to corresponding chemical changes[5]. M. Schœnbein does not admit this inference, and bases his view of "currents of tendency" on the phenomena presented by this body and its non-action with nitric acid[6]. My own results confirm those of M. de la Rive, for by direct experiment I find that the peroxide is acted upon by such bodies as nitric acid. Potash and pure strong nitric acid boiled on peroxide of lead readily dissolved it, forming protonitrate of lead. A dilute nitric acid was made and divided into two portions; one was tested by a solution of sulphuretted hydrogen, and showed no signs of lead: the other was mingled with a little peroxide of lead (1822.) at common temperatures, and after an hour filtered and tested in the same manner, and found to contain plenty of lead.

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[1] Annales de Chimie, 1802, xl. 224.

[2] Ibid. 1835, lx. 164, 171.

[3] Ibid. 1836, lxi. 40; and Bibliothèque Universelle, 1836, i. 152, 158.

[4] Bibliothèque Universelle, 1836, i. 160.

[5] Ibid. 1836, i. 162, 154.

[6] Philosophical Magazine, 1838, xii. 226, 311; and Bibliothèque Universelle, 1838, xiv. 155.

2044. The peroxide of lead is negative to platinum in solutions of common salt and potash, bodies which might be supposed to exert no chemical action on it. But direct experiments show that they do exert sufficient action to produce all the effects. A circumstance in further proof that the current in the voltaic circuit formed by these bodies is chemical in its origin, is the rapid depression in the force of the current produced, after the first moment of immersion.

2045. The most powerful arrangement with peroxide of lead, platinum, and one fluid, was obtained by using a solution of the yellow sulphuret of potassium as the connecting fluid. A convenient mode of making such experiments was to form the peroxide into a fine soft paste with a little distilled water, to cover the lower extremity of a platinum plate uniformly with this paste, using a glass rod for the purpose, and making the coat only thick enough to hide the platinum well, then to dry it well, and finally, to compare that plate with a clean platinum plate in the electrolyte employed. Unless the platinum plate were perfectly covered, local electrical currents (1120.) took place which interfered with the result. In this way, the peroxide is easily shown to be negative to platinum either in the solution of the sulphuret of potassium or in nitric acid. Red-lead gave the same results in both these fluids.

2046. But using this sulphuretted solution, the same kind of proof in support of the chemical theory could be obtained from protoxides as before from the peroxides. Thus, some pure protoxide of lead, obtained from the nitrate by heat and fusion, was applied on the platinum plate (2045.), and found to be strongly negative to metallic platinum in the solution of sulphuret of potassium. White lead applied in the same manner was also found to acquire the same state. Either of these bodies when compared with platinum in dilute nitric acid was, on the contrary, very positive.

2047. The same effect is well shown by the action of oxidized iron. If a plate of iron be oxidized by heat so as to give an oxide of such aggregation and condition as to be acted on scarcely or not at all by the solution of sulphuret, then there is little or no current, such an oxide being as platinum in the solution (1840.). But if it be oxidized by exposure to air, or by being wetted and dried; or by being moistened by a little dilute nitric or sulphuric acid and then washed, first in solution of ammonia or potassa, and afterwards in distilled water and dried; or if it be moistened in solution of potassa, heated in the air, and then washed well in distilled water and dried; such iron associated with platinum and put into a solution of the sulphuret will produce a powerful current until all the oxide is reduced, the iron during the whole time being negative.

2048. A piece of rusty iron in the same solution is powerfully negative. So also is a platinum plate with a coat of protoxide, or peroxide, or native carbonate of iron on it (2045.).

2049. This result is one of those effects which has to be guarded against in the experiments formerly described (1826. 1886.). If what appears to be a clean plate of iron is put into a dilute solution of the sulphuret of potassium, it is first negative to platinum, then neutral, and at last generally feebly positive; if it be put into a strong solution, it is first negative, and then becomes neutral, continuing so. It cannot be cleansed so perfectly with sand-paper, but that when immersed it will be negative, but the more recently and well the plate has been cleansed, the shorter time does this state continue. This effect is due to the instantaneous oxidation of the surface of the iron during its momentary exposure to the atmosphere, and the after reduction of this oxide by the solution. Nor can this be considered an unnatural result to those who consider the characters of iron. Pure iron in the form of a sponge takes fire spontaneously in the air; and a plate recently cleansed, if dipped into water, or breathed upon, or only exposed to the atmosphere, produces an instant smell of hydrogen. The thin film of oxide which can form during a momentary exposure is, therefore, quite enough to account for the electric current produced.

2050. As a further proof of the truth of these explanations, I placed a plate of iron under the surface of a solution of the sulphuret of potassium, and rubbed it there with a piece of wood which had been soaking for some time in the same sulphuret. The iron was then neutral or very slightly positive to platinum connected with it. Whilst in connection with the platinum it was again rubbed with the wood so as to acquire a fresh surface of contact; it did not become negative, but continued in the least degree positive, showing that the former negative current was only a temporary result of the coat of oxide which the iron had acquired in the air.

2051. Nickel appears to be subject to the same action as iron, though in a much slighter degree. All the circumstances were parallel, and the proof applied to iron (2050.) was applied to it also, with the same result.

2052. So all these phenomena with protoxides and peroxides agree in referring the current produced to chemical action; not merely by showing that the current depends upon the action, but also that the *direction* of the current depends upon the direction which the chemical affinity determines the exciting or electromotive anion to take. And it is I think, a most striking circumstance, that these bodies, which when they can and do act chemically produce currents, have not the least power of the kind when *mere contact only* is allowed (1869.), though they are excellent conductors of electricity, and can readily carry the currents formed by other and more effectual means.

2053. With such a mass of evidence for the efficacy and sufficiency of chemical action as that which has been given (1878. 2052.); with so many current circuits without metallic contact (2017.) and so many non-current circuits with (1867.); what reason can there be for referring the effect in the joint cases where both chemical action and contact occur, to contact, or to anything but the chemical force alone? Such a reference appears to me most unphilosophical: it is dismissing a proved and active cause to receive in its place one which is merely hypothetical.

¶ ix. *Thermo-electric evidence.*

2054. The phenomena presented by that most beautiful discovery of Seebeck, thermo-electricity, has occasionally and, also, recently been adduced in proof of the electromotive influence of contact amongst the metals, and such like solid conductors[1] (1809. 1867.). A very brief consideration is, I think, sufficient to show how little support these phenomena give to the theory in question.

2055. If the contact of metals exert any exciting influence in the voltaic circuit, then we can hardly doubt that thermo-electric currents are due to the same force; i. e. to disturbance, by local temperature, of the balanced forces of the different contacts in a metallic or similar circuit. Those who quote thermo effects as proofs of the effect of contact must, of course, admit this opinion.

2056. Admitting contact force, we may then assume that heat either increases or diminishes the electromotive force of contact. For if in fig. 16. A be antimony and B bismuth, heat applied at x causes a current to pass in the direction of the arrow; if it be assumed that bismuth in contact with antimony tends to become positive and the antimony negative, then heat diminishes the effect; but if it be supposed that the tendency of bismuth is to become negative, and of antimony positive, then heat increases the effect. How we are to decide which of these two views is the one to be adopted, does not seem to me clear; for nothing in the thermo-electric phenomena alone can settle the point by the galvanometer.

2057. If for that purpose we go to the voltaic circuit, there the situation of antimony and bismuth varies according as one or another fluid conductor is used (2012.). Antimony, being negative to bismuth with the acids, is positive to it with an alkali or sulphuret of potassium; still we find they come *nearly together* in the midst of the metallic series. In the thermo series, on the contrary, their position is at the *extremes*, being as different or as much opposed to each other as they can be. This difference was long ago pointed out by Professor Cumming[2]: how is it consistent with the contact theory of the voltaic pile?

2058. Again, if silver and antimony form a thermo circle (fig.17.), and the junction x be heated, the current there is from the silver to the antimony. If silver and bismuth form a thermo series (fig. 18.), and the junction *x* be heated, the current is from the bismuth to the silver; and assuming that heat increases the force of contact (2056.), these results will give the direction of contact force between these metals, *antimony* ←—— *silver*, and *bismuth* ——→ *silver*. But in the voltaic series the current is *from the silver* to both the antimony and bismuth at their points of contact, whenever dilute sulphuric or nitric acid, or strong nitric acid, or solution of potassa (2012.) are used; so that metallic contact, like that in the thermo circle, can at all events have *very little* to do here. In the yellow sulphuret of potassium the current is from both antimony and bismuth *to the silver* at their contacts, a result equally inconsistent with the thermo effect as the former. When the colourless hydrosulphuret of potassium is used to complete the voltaic circle, the current is from bismuth to silver, and from silver to antimony at their points of contact; whilst, with strong muriatic acid, precisely the reverse direction occurs, for it is from silver to bismuth, and from antimony to silver at the junctions.

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[1] See Fechner's words. Philosophical Magazine, 1838, xiii. p. 206.

[2] Annals of Philosophy, 1823, vi. 177.

2059. Again;—by the heat series copper gives a current to gold; tin and lead give currents to copper, rhodium, or gold; zinc gives one to antimony, or iron, or even plumbago; and bismuth gives one to nickel, cobalt, mercury, silver, palladium, gold, platinum, rhodium, and plumbago; at the *point of contact* between the metals:—currents which are just the reverse of those produced by the same metals, when formed into voltaic circuits and excited by the ordinary acid solutions (2012.).

2060. These, and a great number of other discrepancies, appear by a comparison, according to theory, of thermo contact and voltaic contact action, which can only be accounted for by assuming a specific effect of the contact of water, acids, alkalies, sulphurets, and other exciting electrolytes, for each metal; this assumed contact force being not only unlike thermo-metallic contact, in not possessing a balanced state in the complete circuit at uniform temperatures, but, also, having no relation to it as to the *order* of the metals employed. So bismuth and antimony, which are far apart in thermo-electric order, mnst have this extra character of acid contact very greatly developed in an opposite direction as to its result, to render them only a feeble voltaic combination with each other: and with respect to silver, which stands between tin and zinc thermo-electrically, not only must the same departure be required, but how great must the effect of this, its incongruous contact, be, to overcome so completely as it does, and even powerfully reverse the differences which the metals (according to the contact theory) tend to produce!

2061. In further contrast with such an assumption, it must be remembered that, though the series of thermo-electric bodies is different from the usual voltaic order (2012.), it is perfectly consistent with itself, i. e. that if iron and antimony be weak with each other, and bismuth be strong with iron, it will also be strong with antimony. Also that if the electric current pass from bismuth to rhodium at the hot junction, and also from rhodium to antimony at the hot junction, it will pass far more powerfully from bismuth to antimony at the heated junction. To be at all consistent with this simple and true relation, sulphuric acid should not be strongly energetic with iron or tin and weakly so with silver, as it is in the voltaic circuit, since these metals are not far apart in the thermo series: nor should it be nearly alike to platinum and gold voltaically, since they are far apart in the thermo series.

2062. Finally, in the thermo circuit there is that relation to heat which shows that for every portion of electric force evolved, there is a corresponding change in another force, or form of force, namely heat, able to account for it; this, the united experiments of Seebeck and Peltier have shown. But contact force is a force which has to produce something from nothing, a result of the contact theory which can be better stated a little further on (2069. 2071. 2073.).

2063. What evidence then for mere contact excitement, derivable from the facts of thermo-electricity, remains, since the power must thus be referred to the acid or other electrolyte used (2060.) and made, not only to vary uncertainly for each metal, but to vary also in direct conformity with the variation of chemical action (1874. 1956. 1992. 2006. 2014.)?

2064. The contact theorist seems to consider that the advocate of the chemical theory is called upon to account for the phenomena of thermo-electricity. I cannot perceive that Seebeck's circle has any relation to the voltaic pile, and think that the researches of Becquerel[1] are quite sufficient to authorize that conclusion.

¶ x. *Improbable nature of the assumed contact force.*

2065. I have thus given a certain body of experimental evidence and consequent conclusions, which seem to me fitted to assist in the elucidation of the disputed point, in addition to the statements and arguments of the great men who have already advanced their results and opinions in favour of the chemical theory of excitement in the voltaic pile, and against that of contact. I will conclude by adducing a further argument founded upon the, to me, unphilosophical nature of the force to which the phenomena are, by the contact theory, referred.

2066. It is assumed by the theory (1802.) that where two dissimilar metals (or rather bodies) touch, the dissimilar particles act on each other, and induce opposite states. I do not deny this, but on the contrary think, that in many cases such an effect takes place between contiguous particles; as for instance, preparatory to action in common chemical phenomena, and also preparatory to that act of chemical combination which, in the voltaic circuit, causes the current (1738. 1743.).

2067. But the contact theory assumes that these particles, which have thus by their mutual action acquired opposite electrical states, can discharge these states one to the other, and yet remain in the state they were first in, being *in every point* entirely unchanged by what has previously taken place. It assumes also that the particles, being by their mutual action rendered plus and minus, can, whilst under this inductive action, discharge to particles of like matter with themselves and so produce a current.

2068. This is in no respect consistent with known actions. If in relation to chemical phenomena we take two substances, as oxygen and hydrogen, we may conceive that two particles, one of each, being placed together and heat applied, they induce contrary states in their opposed surfaces, according, perhaps, to the view of Berzelius (1739.), and that these states becoming more and more exalted end at last in a mutual discharge of the forces, the particles being ultimately found combined, and unable to repeat the effect. Whilst they are under induction and before the final action comes on, they cannot spontaneously lose that state; but by removing the *cause* of the increased inductive effect, namely the heat, the effect itself can be lowered to its first condition. If the acting particles are involved in the constitution of an electrolyte, then they can produce current force (921. 924.) proportionate to the amount of chemical force consumed (868.).

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[1] Annales de Chimie, 1829, xli. 355. xlvi. 275.

2069. But the contact theory, which is obliged, according to the facts, to admit that the acting particles are not changed (1802. 2067.) (for otherwise it would be the chemical theory), is constrained to admit also, that the force which is able to make two particles assume a certain state in respect to each other, is unable to make them *retain* that state; and so it virtually denies the great principle in natural philosophy, that cause and effect are equal (2071.). If a particle of platinum by contact with a particle of zinc willingly gives of its own electricity to the zinc, because this by its presence tends to make the platinum assume a negative state, why should the particle of platinum take electricity from any other particle of platinum behind it, since that would only tend to destroy the very state which the zinc has just forced it into? Such is not the case in common induction; (and Marianini admits that the effect of contact may take place through air and measurable distances[1]); for there a ball rendered negative by induction, will not take electricity from surrounding bodies, however thoroughly we may uninsulate it; and if we force electricity into it, it will, as it were, be spurned back again with a power equivalent to that of the inducing body.

2070. Or if it be supposed rather, that the zinc particle, by its inductive action, tends to make the platinum particle positive, and the latter, being in connection with the earth by other platinum particles, calls upon them for electricity, and so acquires a positive state; why should it discharge that state to the zinc, the very substance, which, making the platinum assume that condition, ought of course to be able to sustain it? Or again, if the zinc tends to make the platinum particle positive, why should not electricity go to the platinum *from the zinc*, which is as much in contact with it as its neighbouring platinum particles are? Or if the zinc particle in contact with the platinum tends to become positive, why does not electricity flow to it from the zinc particles behind, as well as from the platinum[1]? There is no sufficient probable or philosophic cause assigned for the assumed action; or reason given why one or other of the consequent effects above mentioned should not take place: and, as I have again and again said, I do not know of a single fact, or case of contact current, on which, in the absence of such probable cause, the theory can rest.

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[1] Memorie della Società Italiana in Modena, 1837, xxi. 232, 233, &c.

2071. The contact theory assumes, in fact, that a force which is able to overcome powerful resistance, as for instance that of the conductors, good or bad, through which the current passes, and that again of the electrolytic action where bodies are decomposed by it, can arise out of nothing; that, without any change in the acting matter or the consumption of any generating force, a current can be produced which shall go on for ever against a constant resistance, or only be stopped, as in the voltaic trough, by the ruins which its exertion has heaped up in its own course. This would indeed be *a creation of power*, and is like no other force in nature. We have many processes by which the form of the power may be so changed that an apparent *conversion* of one into another takes place. So we can change chemical force into the electric current, or the current into chemical force. The beautiful experiments of Seebeck and Peltier show the convertibility of heat and electricity; and others by Œrsted and myself show the convertibility of electricity and magnetism. But in no cases, not even those of the Gymnotus and Torpedo (1790.), is there a pure creation of force; a production of power without a corresponding exhaustion of something to supply it[2].

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[1] I have spoken, for simplicity of expression, as if one metal were active and the other passive in bringing about these induced states, and not, as the theory implies, as if each were mutually subject to the other. But this makes no difference in the force of the argument; whilst an endeavour to state fully the joint changes on both sides, would rather have obscured the objections which arise, and which yet are equally strong in either view.

[2] (*Note*, March 29, 1840.)—I regret that I was not before aware of most

2072. It should ever be remembered that the chemical theory sets out with a power the existence of which is pre-proved, and then follows its variations, rarely assuming anything which is not supported by some corresponding simple chemical fact. The contact theory sets out with an assumption, to which it adds others as the cases require, until at last the contact force, instead of being the firm unchangeable thing at first supposed by Volta, is as variable as chemical force itself.

2073. Were it otherwise than it is, and were the contact theory true, then, as it appears to me, the equality of cause and effect must be denied (2069.). Then would the perpetual motion also be true; and it would not be at all difficult, upon the first given case of an electric current by contact alone, to produce an electro-magnetic arrangement, which, as to its principle, would go on producing mechanical effects for ever.

Royal Institution,
December 26, 1839.

NOTE.

2074. In a former series (925, &c.) I have said that I do not think any part of the electricty of the voltaic pile is due to the

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important evidence for this philosophical argument, consisting of the opinion of Dr. Roget, given in his Treatise on Galvanism in the Library of Useful Knowledge, the date of which is January 1829. Dr. Roget is, upon the facts of the science, a supporter of the chemical theory of excitation; but the striking passage I desire now to refer to, is the following, at § 113. of the article Galvanism. Speaking of the voltaic theory of contact, he says, "Were any further reasoning necessary to overthrow it, a forcible argument might be drawn from the following consideration. If there could exist a power having the property ascribed to it by the hypothesis, namely, that of giving continual impulse to a fluid in one constant direction, without being exhausted by its own action, it would differ essentially from all the other known powers in nature. All the powers and sources of motion, with the operation of which we are acquainted, when producing their peculiar effects, are expended in the same proportion as those effects are produced; and hence arises the impossibility of obtaining by their agency a perpetual effect; or, in other words, a perpetual motion. But the electromotive force ascribed by Volta to the metals when in contact is a force which, as long as a free course is allowed to the electricity it sets in motion, is never expended, and continues to be excited with undiminished power, in the production of a never-ceasing effect. Against the truth of such a supposition, the probabilities are all but infinite."—*Roget.*

combination of the oxide of zinc with the sulphuric acid used, and that I agreed so far with Sir Humphry Davy in thinking that acids and alkalies did not in combining evolve electricity in large quantity when they were not parts of electrolytes.

This I would correct; for I think that Becquerel's pile is a perfect proof that when acid and alkali combine an electric current is produced[1].

I perceive that Dr. Mohr of Coblentz appears to have shown that it is only nitric acid which amongst acids can in combining with alkalies produce an electric current[2].

For myself, I had made exception of the hydracids (929.) on theoretical grounds. I had also admitted that oxyacids when in solution might in such cases produce small currents of electricity (928. and *Note.*); and Jacobi says that in Becquerel's improved acid and alkaline pile, it is not above a thirtieth part of the whole power which appears as current. But I now wish to say, that though in the voltaic battery, dependent for its power on the oxidizement of zinc, I do not think that the *quantity* of electricity is at all increased or affected by the combination of the oxide with the acid (933. 945.), still the latter circumstance cannot go altogether for nothing. The researches of Mr. Daniell on the nature of compound electrolytes[3] ties together the electrolyzation of a salt and the water in which it is dissolved, in such a manner as to make it almost certain that, in the corresponding cases of the *formation* of a salt at the place of excitement in the voltaic circuit, a similar connection between the water and the salt formed must exist: and I have little doubt that the joint action of water, acids, and bases, in Becquerel's battery, in Daniell's electrolyzations, and at the zinc in the ordinary active pile, are, in principle, closely connected together.

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[1] Bibliothèque Universelle, 1838, xiv. 129. 171. Comptes Rendus, i. p. 455. Annales de Chimie, 1827, xxxv. 122.

[2] Philosophical Magazine, 1838, xiii. p. 382; or Poggendorf's Annalen, xlii. p. 76.

[3] Philosophical Transactions, 1839, p. 97.

EIGHTEENTH SERIES.

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§ 25. *On the electricity evolved by the friction of water and steam against other bodies.*

2075. TWO years ago an experiment was described by Mr. Armstrong and others[1], in which the issue of a stream of high pressure steam into the air produced abundance of electricity. The source of the electricity was not ascertained, but was supposed to be the evaporation or change of state of the water, and to have a direct relation to atmospheric electricity. I have at various times since May of last year been working upon the subject, and though I perceive Mr. Armstrong has, in recent communications, anticipated by publication some of the facts which I also have obtained, the Royal Society may still perhaps think a compressed account of my results and conclusions, which include many other important points, worthy its attention.

2076. The apparatus I have used was not competent to furnish me with much steam or a high pressure, but I found it sufficient for my purpose, which was the investigation of the effect and its cause, and not necessarily an increase of the electric development. Mr. Armstrong, as is shown by a recent paper, has well effected the latter[2]. The boiler I used, belonging to the London Institution, would hold about ten gallons of water, and allow the evaporation of five gallons. A pipe 4½ feet long was attached to it, at the end of which was a large stop-cock and a metal globe, of the capacity of thirty-two cubic inches, which I will call the *steam-globe*, and to this globe, by its mouth-piece, could be attached various forms of apparatus, serving as vents for the issuing steam[1]. Thus a cock could be connected with the steam-globe, and this cock be used as the experimental steam-passage; or a wooden tube could be screwed in; or a small metal or glass tube put through a good cork, and the cork screwed in; and in these cases the steam way of the globe and tube leading to the boiler was so large, that they might be considered as part of the boiler, and these terminal passages as the obstacles which, restraining the issue of steam, produced any important degree of friction.

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[1] Philosophical Magazine, 1840, vol. xvii. pp. 370, 452, &c.

[2] Ibid. 1843, vol. xxii. p. 1.

2077. Another issue piece consisted of a metal tube terminated by a metal funnel, and of a cone advancing by a screw more or less into the funnel, so that the steam as it rushed forth beat against the cone (Plate I. fig. 2.); and this cone could either be electrically connected with the funnel and boiler, or be insulated.

2078. Another terminal piece consisted of a tube, with a stop-cock and feeder attached to the top part of it, by which any fluid could be admitted into the passage, and carried on with the steam (fig. 3.).

2079. In another terminal piece, a small cylindrical chamber was constructed (fig. 4.) into which different fluids could be introduced, so that, when the cocks were opened, the steam passing on from the steam-globe (2076.) should then enter this chamber and take up anything that was there, and so proceed with it into the final passage, or out against the cone (2077.), according as the apparatus had been combined together. This little chamber I will always call C.

2080. The pressure at which I worked with the steam was from eight to thirteen inches of mercury, never higher than thirteen inches, or about two-fifths of an atmosphere.

2081. The boiler was insulated on three small blocks of lac, the chimney being connected by a piece of funnel-pipe removable at pleasure. Coke and charcoal were burnt, and the insulation was so good, that when the boiler was attached to a gold-leaf electrometer and charged purposely, the divergence of the leaves did not alter either by the presence of a large fire, or the abundant escape of the results of the combustion.

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[1] This globe and the pieces of apparatus are represented upon a scale of one-fourth in the Plate belonging to this paper.

2082. When the issuing steam produces electricity, there are two ways of examining the effect: either the insulated boiler may be observed, or the steam may be examined, but these states are always contrary one to the other. I attached to the boiler both a gold-leaf and a discharging electrometer, the first showed any charge short of a spark, and the second by the number of sparks in a given time carried on the measurement of the electricity evolved. The state of the steam may be observed either by sending it through an insulated wide tube in which are some diaphragms of wire gauze, which serves as a discharger to the steam, or by sending a puff of it near an electrometer when it acts by induction; or by putting wires and plates of conducting matter in its course, and so discharging it. To examine the state of the boiler or substance against which the steam is excited, is far more convenient, as Mr. Armstrong has observed, than to go for the electricity to the steam itself; and in this paper I shall give the state of the former, unless it be otherwise expressed.

2083. Proceeding to the cause of the excitation, I may state first that I have satisfied myself it is not due to evaporation or condensation, nor is it affected by either the one or the other. When the steam was at its full pressure, if the valve were suddenly raised and taken out, no electricity was produced in the boiler, though the evaporation was for the time very great. Again, if the boiler were charged by excited resin before the valve was opened, the opening of the valve and consequent evaporation did not affect this charge. Again, having obtained the power of constructing steam passages which should give either the positive or the negative, or the neutral state (2102. 2110. 2117.), I could attach these to the steam way, so as to make the boiler either positive, or negative, or neutral at pleasure with the same steam, and whilst the evaporation for the whole time continued the same. So that the excitation of electricity is clearly independent of the evaporation or of the change of state.

2084. The issue of *steam alone* is not sufficient to evolve electricity[1]. To illustrate this point I may say that the cone apparatus (2077.) is an excellent exciter: so also is a boxwood tube (2102. fig. 5.) soaked in water, and screwed into the steam-globe. If with either of these arrangements, the steam-globe (fig. 1.) be empty of water, so as to catch and retain that which is condensed from the steam, then after the first moment (2089.), and when the apparatus is hot, the issuing steam excites no electricity; but when the steam-globe is filled up so far that the rest of the condensed water is swept forward with the steam, abundance of electricity appears. If then the globe be emptied of its water, the electricity ceases; but upon filling it up to the proper height, it immediately reappears in full force. So when the feeder apparatus (2078.) was used, whilst there was no water in the passage-tube, there was no electricity; but on letting in water from the feeder, electricity was immediately evolved.

2085. The electricity is due entirely to the friction of the particles of water which the steam carries forward against the surrounding solid matter of the passage, or that which, as with the cone (2077.), is purposely opposed to it, and is in its nature like any other ordinary case of excitement by friction. As will be shown hereafter (2130. 2132.), a very small quantity of water properly rubbed against the obstructing or interposed body, will produce a very sensible proportion of electricity.

2086. Of the many circumstances affecting this evolution of electricity, there are one or two which I ought to refer to here. Increase of pressure (as is well illustrated by Mr. Armstrong's experiments) greatly increases the effect, simply by rubbing the two exciting substances more powerfully together. Increase of pressure will sometimes change the positive power of a passage to negative; not that it has power of itself to change the quality of the passage, but as will be seen presently (2108.), by carrying off that which gave the positive power; no increase of pressure, as far as I can find, can change the negative power of a given passage to positive. In other phenomena hereafter to be described (2090. 2105.), increase of pressure will no doubt have its influence; and an effect which has been decreased, or even annihilated (as by the addition of substances to the water in the steam-globe, or to the issuing current of water and steam), may, no doubt, by increase of pressure be again developed and exalted.

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[1] Mr. Armstrong has also ascertained that water is essential to a high development. Phil. Mag. 1843, vol. xxii. p. 2.

2087. The shape and form of the exciting passage has great influence, by favouring more or less the contact and subsequent separation of the particles of water and the solid substance against which they rub.

2088. When the mixed steam and water pass through a tube or stop-cock (2076.), they may issue, producing either a hissing smooth sound, or a rattling rough sound[1]; and with the cone apparatus (2077. fig. 2.), or certain lengths of tube, these conditions alternate suddenly. With the smooth sound little or no electricity is produced; with the rattling sound plenty. The rattling sound accompanies that irregular rough vibration, which casts the water more violently and effectually against the substance of the passage, and which again causes the better excitation. I converted the end of the passage into a steam-whistle, but this did no good.

2089. If there be no water in the steam-globe (2076.), upon opening the steam-cock the *first effect* is very striking; a good excitement of electricity takes place, but it very soon ceases. This is due to water condensed in the cold passages, producing excitement by rubbing against them. Thus, if the passage be a stop-cock, whilst cold it excites electricity with what is supposed to be steam only; but as soon as it is hot, the electricity ceases to be evolved. If, then, whilst the steam is issuing, the cock be cooled by an insulated jet of water, it resumes its power. If, on the other hand, it be made hot by a spirit-lamp before the steam be let on, then there is *no* first effect. On this principle, I have made an exciting passage by surrounding one part of an exit tube with a little cistern, and putting spirits of wine or water into it.

2090. We find then that particles of water rubbed against other bodies by a current of steam evolve electricity. For this purpose, however, it is not merely water but *pure* water which must be used. On employing the feeding apparatus (2078.), which supplied the rubbing water to the interior of the steam passage, I found, as before said, that with steam only I obtained no electricity (2084.). On letting in distilled water, abundance of electricity was evolved; on putting a small crystal of sulphate of soda, or of common salt into the water, the evolution ceased entirely. Re-employing distilled water, the electricity appeared again; on using the common water supplied to London, it was unable to produce it.

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[1] Messrs. Armstrong and Schafhaeutl have both observed the coincidence of certain sounds or noises with the evolution of the electricity.

2091. Again, using the steam-globe (2076.), and a box-wood tube (2102.) which excites well if the water distilling over from the boiler be allowed to pass with the steam, when I put a small crystal of sulphate of soda, of common salt, or of nitre, or the smallest drop of sulphuric acid, into the steam-globe with the water, the apparatus was utterly ineffective, and no electricity could be produced. On withdrawing such water and replacing it by distilled water, the excitement was again excellent: on adding a very small portion of any of these substances, it ceased; but upon again introducing pure water it was renewed.

2092. Common water in the steam-globe was powerless to excite. A little potash added to distilled water took away all its power; so also did the addition of *any* of those saline or other substances which give conducting power to water.

2093. The effect is evidently due to the water becoming so good a conductor, that upon its friction against the metal or other body, the electricity evolved can be immediately discharged again, just as if we tried to excite lac or sulphur by flannel which was damp instead of dry. It shows very clearly that the exciting effect, when it occurs, is due to water and not to the passing steam.

2094. As ammonia increases the conducting power of water only in a small degree (554.), I concluded that it would not take away the power of excitement in the present case; accordingly on introducing some to the pure water in the globe, electricity was still evolved though the steam of vapour and water was able to redden moist turmeric paper. But the addition of a very small portion of dilute sulphuric acid, by forming sulphate of ammonia, took away all power.

2095. When in any of these cases, the steam-globe contained water which could not excite electricity, it was beautiful to observe how, on opening the cock which was inserted into the steam-pipe before the steam-globe, fig. 1. (the use of which was to draw off the water condensed in the pipe before it entered the steam-globe), electricity was instantly evolved; yet a few inches further on the steam was quite powerless, because of the small change in the quality of the water over which it passed, and which it took with it.

2096. When a wooden or metallic tube (2076.) was used as the exciting passage, the application of solution of salts to the outside and end of the tube in no way affected the evolution. But when a wooden cone (2077.) was used, and that cone moistened with the solutions, there was no excitement on first letting out the steam, and it was only as the solution was washed away that the power appeared; soon rising, however, to its full degree.

2097. Having ascertained these points respecting the necessity of water and its purity, the next for examination was the influence of the substance against which the stream of steam and water rubbed. For this purpose I first used cones (2077.) of various substances, either insulated or not, and the following, namely, brass, box-wood, beech-wood, ivory, linen, kerseymere, white silk, sulphur, caoutchouc, oiled silk, japanned leather, melted caoutchouc and resin, all became negative, causing the stream of steam and water to become positive. The fabrics were applied stretched over wooden cones. The melted caoutchouc was spread over the surface of a box-wood or a linen cone, and the resin cone was a linen cone dipped in a strong solution of resin in alcohol, and then dried. A cone of wood dipped in oil of turpentine, another cone soaked in olive oil, and a brass cone covered with the alcholic solution of resin and dried, were at first inactive, and then gradually became negative, at which time the oil of turpentine, olive-oil and resin were found cleared off from the parts struck by the stream of steam and water. A cone of kerseymere, which had been dipped in alcoholic solution of resin and dried two or three times in succession, was very irregular, becoming positive and negative by turns, in a manner difficult to comprehend at first, but easy to be understood hereafter (2113.).

2098. The end of a rod of shell-lac was held a moment in the stream of steam and then brought near a gold-leaf electrometer: it was found excited negatively, exactly as if it had been rubbed with a piece of flannel. The corner of a plate of sulphur showed the same effect and state when examined in the same way.

2099. Another mode of examining the substance rubbed was to use it in the shape of wires, threads or fragments, holding them by an insulating handle in the jet, whilst they were connected with a gold-leaf electrometer. In this way the following substances were tried:—

Platinum,
Copper,
Iron,
Zinc,
Sulphuret of copper,
Linen,
Cotton,
Silk,
Worsted,
Wood,
Horse-hair,
Bear's hair,
Flint glass,
Green glass,
Quill,
Ivory,
Shell-lac on silk,
Sulphur on silk,
Sulphur in piece,
Plumbago,
Charcoal,
Asbestus,
Cyanite,
Hæmatite,
Rock-crystal,
Orpiment,
Sulphate of baryta,
Sulphate of lime,
Carbonate of lime,
Fluor-spar.

All these substances were rendered negative, though not in the same degree. This apparent difference in degree did not depend *only* upon the specific tendency to become negative, but also upon the conducting power of the body itself, whereby it gave its charge to the electrometer; upon its tendency to become wet (which is very different, for instance in shell-lac or quill, to that of glass or linen), by which its conducting quality was affected; and upon its size or shape. Nevertheless I could distinguish that bear's hair, quill and ivory had very feeble powers of exciting electricity as compared to the other bodies.

2100. I may make here a remark or two upon the introduction of bodies into the jet. For the purpose of preventing condensation on the substance, I made a platinum wire white-hot by an insulated voltaic battery, and introduced it into the jet: it was quickly lowered in temperature by the stream of steam and water to 212°, but of course could never be below the boiling-point. No difference was visible between the effect at the first instant of introduction or any other time. It was always instantly electrified and negative.

2101. The threads I used were stretched across a fork of stiff wire, and the middle part of the thread was held in the jet of vapour. In this case, the string or thread, if held exactly in the middle of the jet and looked at end-ways to the thread, was seen to be still, but if removed the least degree to the right or left of the axis of the stream it (very naturally) vibrated, or rather rotated, describing a beautiful circle, of which the axis of the stream was the tangent: the interesting point was to observe, that when the thread rotated, travelling as it were with the current, there was little or no electricity evolved, but that when it was nearly or quite stationary there was abundance of electricity, thus illustrating the effect of friction.

2102. The difference in the quality of the substances above described (2099.) gives a valuable power of arrangement at the jet. Thus if a metal, glass, or wood tube[1] (2076.) be used for the steam issue, the boiler is rendered well negative and the steam highly positive; but if a quill tube or, better still, an ivory tube be used, the boiler receives scarcely any charge, and the stream of steam is also in a neutral state. This result not only assists in proving that the electricity is not due to evaporation, but is also very valuable in the experimental inquiry. It was in such a neutral jet of steam and water that the excitation of the bodies already described (2099.) was obtained.

2103. Substances, therefore, may be held either in the neutral jet from an ivory tube, or in the positive jet from a wooden or metal tube; and in the latter case effects occurred which, if not understood, would lead to great confusion. Thus an insulated wire was held in the stream issuing from a glass or metal tube, about half an inch from the mouth of the tube, and was found to be unexcited: on moving it in one direction a little further off, it was rendered positive; on moving it in the other direction, nearer to the tube, it was negative. This was simply because, when near the tube in the forcible part of the current, it was excited and rendered negative, rendering the steam and water more positive than before, but that when further off, in a quieter part of the current, it served merely as a discharger to the electricity previously excited in the exit tube, and so showed the same state with it. Platinum, copper, string, silk, wood, plumbago, or any of the substances mentioned above (2099.), excepting quill, ivory and bear's hair, could, in this way, be made to assume either one state or the other, according as they were used as exciters or dischargers, the difference being determined by their place in the stream. A piece of fine wire gauze held across the issuing jet shows the above effect very beautifully; the difference of an eighth of an inch either way from the neutral place will change the state of the wire gauze.

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[1] A box-wood tube, 3 inches long and $\frac{1}{8}$th of an inch inner diameter, well soaked in distilled water and screwed into the steam-globe, is an admirable exciter.

2104. If, instead of an excited jet of steam and water (2103.), one issuing from an ivory tube (2102.), and in the neutral state be used, then the wires, &c. can no longer be made to assume both states. They may be excited and rendered negative (2099.), but at no distance can they become dischargers, or show the positive state.

2105. We have already seen that the presence of a very minute quantity of matter able to give conducting power to the water took away all power of excitation (2090, &c.) up to the highest degree of pressure, i. e. of mechanical friction that I used (2086.); and the next point was to ascertain whether it would be so for all the bodies rubbed by the stream, or whether differences in degree would begin to manifest themselves. I therefore tried all these bodies again, at one time adding about two grains of sulphate of soda to the four ounces of water which the steam-globe retained as a constant quantity when in regular action, and at another time adding not a fourth of this quantity of sulphuric acid (2091.). In both cases all the substances (2099.) remained entirely unexcited and neutral. Very probably, great increase of pressure might have developed some effect (2086.).

2106. With dilute sulphuric acid in the steam-globe, varying from extreme weakness to considerable sourness, I used tubes and cones of zinc, but could obtain *no trace* of electricity.

Chemical action, therefore, appears to have nothing to do with the excitement of electricity by a current of steam.

2107. Having thus given the result of the friction of the steam and water against so many bodies, I may here point out the remarkable circumstance of water being *positive* to them all. It very probably will find its place above all other substances, even cat's hair and oxalate of lime (2131.). We shall find hereafter, that we have power, not merely to prevent the jet of steam and water from becoming positive, as by using an ivory tube (2102.), but also of reducing its own power when passing through or against such substances as wood, metal, glass, &c. Whether, with a jet so reduced, we shall still find amongst the bodies above mentioned (2099.) some that can render the stream positive and others that can make it negative, is a question yet to be answered.

2108. Advancing in the investigation, a new point was to ascertain what other bodies, than water, would do if their particles were carried forward by the current of steam. For this purpose the feeding apparatus (2078.) was mounted and charged with oil of turpentine, to be let in at pleasure to the steam-exit passage. At first the feeder stop-cock was shut, and the issuing steam and water made the boiler negative. On letting down the oil of turpentine, this state was instantly changed, the boiler became powerfully positive, and the jet of steam, &c. as strongly negative. Shutting off the oil of turpentine, this state gradually fell, and in half a minute the boiler was negative, as at first. The introduction of more oil of turpentine instantly changed this to positive, and so on with perfect command of the phenomena.

2109. Removing the feeder apparatus and using only the steam-globe and a wooden exit tube (2076.), the same beautiful result was obtained. With pure water in the globe the boiler was negative, and the issuing steam, &c. positive; but a drop or two of oil of turpentine, introduced into the steam-globe with the water, instantly made the boiler positive and the issuing stream negative. On using the little interposed chamber C (2079.), the effects were equally decided. A piece of clean new sail-cloth was formed into a ring, moistened with oil of turpentine and placed in the box; as long as a trace of the fluid remained in the box the boiler was positive and the issuing stream negative.

2110. Thus the positive or negative state can be given at pleasure, either to the substance rubbed or to the rubbing stream; and with respect to this body, oil of turpentine, its perfect and ready dissipation by the continuance of the passage of the steam soon causes the new effect to cease, yet with the power of renewing it in an instant.

2111. With olive oil the same general phenomena were observed, i. e. it made the stream of steam, &c. *negative*, and the substance rubbed by it *positive*. But from the comparative fixedness of oil, the state was much more permanent, and a very little oil introduced into the steam-globe (2076.), or into the chamber C (2079.), or into the exit tube, would make the boiler positive for a long time. It required, however, that this oil should be in such a place that the steam stream, after passing by it, should rub against other matter. Thus, on using a wooden tube (2076. 2102.) as the exciter, if a little oil were applied to the inner termination, or that at which the steam entered it, the tube was made positive and the issuing steam negative; but if the oil were applied to the outer termination of the tube, the tube had its ordinary negative state, as with pure water, and the issuing steam was positive.

2112. Water is essential to this excitation by fixed oil, for when the steam-globe was emptied of water, and yet oil left in it and in the passages, there was no excitement. The first effect (2089.) it is true was one of excitement, and it rendered the boiler positive, but that was an effect due to the water condensed in the passage, combined with the action of the oil. Afterwards when all was hot, there was no evolution of electricity.

2113. I tried many other substances with the chamber C and other forms of apparatus, using the wet wooden tube (2102.) as the place and substance by which to excite the steam stream. Hog's-lard, spermaceti, bees'-wax, castor-oil, resin applied dissolved in alcohol; these, with olive-oil, oil of turpentine, and oil of laurel, all rendered the boiler positive, and the issuing steam negative. Of substances which seemed to have the reverse power, it is doubtful if there are any above water.

Sulphuret of carbon, naphthaline, sulphur, camphor, and melted caoutchouc, occasionally seemed in strong contrast to the former bodies, making the boiler very negative, but on trying pure water immediately after, it appeared to do so quite as powerfully. Some of the latter bodies with oil-gas liquid, naphtha and caoutchoucine, gave occasionally variable results, as if they were the consequence of irregular and complicated effects. Indeed, it is easy to comprehend, that according as a substance may adhere to the body rubbed, or be carried off by the passing stream, exchanging its mechanical action from rubbed to rubber, it should give rise to variable effects; this, I think, was the case with the cone and resin before referred to (2097.).

2114. The action of salts, acids, &c., when present in the water to destroy its effect, I have already referred to (2090, &c.). In addition, I may note that sulphuric ether, pyroxylic spirit, and boracic acid did the same.

2115. Alcohol seemed at the first moment to render the boiler positive. Half alcohol and half water rendered the boiler negative, but much less so than pure water.

2116. It must be considered that a substance having the reverse power of water, but only in a small degree, may be able to indicate that property merely by diminishing the power of water. This diminution of power is very different in its cause to that dependent on increasing the conducting power of the water, as by saline matter (2090.), and yet the apparent effect will be the same.

2117. When it is required to render the issuing steam permanently negative, the object is very easily obtained. A little oil or wax put into the steam-globe (2076.), or a thick ring of string or canvas soaked in wax, or solution of resin in alcohol, and introduced into the box C (2079.), supplies all that is required. By adjusting the application it is easy to neutralize the power of the water, so that the issuing stream shall neither become electric, nor cause that to be electrified against which it rubs.

2118. We have arrived, therefore, at three modes of rendering the jet of steam and water neutral, namely, the use of an ivory or quill tube (2102.), the presence of substances in the water (2090, &c.), and the neutralization of its natural power by the contrary force of oil, resin, &c. &c.

2119. In experiments of the kind just described an ivory tube cannot be used safely with acid or alkalies in the steam-globe, for they, by their chemical action on the substance of the tube, in the evolution or solution of the oily matter for instance, change its state and make its particular power of excitement very variable. Other circumstances also powerfully affect it occasionally (2144.).

2120. A very little oil in the rubbing passages produces a great effect, and this at first was a source of considerable annoyance, by the continual occurrence of unexpected results; a portion may lie concealed for a week together in the thread of an unsuspected screw, and yet be sufficient to mar the effect of every arrangement. Digesting and washing with a little solution of alkali, and avoiding all oiled washers, is the best way in delicate experiments of evading the evil. Occasionally I have found that a passage, which was in some degree persistently negative, from a little melted caoutchouc, or positive from oil, resin, &c., might be cleared out thoroughly by letting oil of turpentine be blown through it; it assumed for a while the positive state, but when the continuance of steam had removed that (2110.), the passage appeared to be perfectly clear and good and in its normal condition.

2121. I now tried the effect of oil, &c. when a little saline matter or acid was added to the water in the steam-globe (2090, &c.), and found that when the water was in such a state as to have no power of itself, still oil of turpentine, or oil, or resin in the box C, showed their power, in conjunction with such water, of rendering the boiler positive, but their power appeared to be reduced: increase of the force of steam, as in all other cases, would, there is little doubt, have exalted it again. When alkali was in the steam-globe, oil and resin lost very much of their power, and oil of turpentine very little. This fact will be important hereafter (2126.).

2122. We have seen that the action of such bodies as oil introduced into the jet of steam changed its power (2108.), but it was only by experiment we could tell whether this change was to such an extent as to alter the electricity for few or many of the bodies against which the steam stream rubbed. With olive oil in the box C, *all* the insulated cones before enumerated (2097.) were made positive. With acetic acid in the steam-globe all were made neutral (2091.). With resin in the box C (2113.), all the substances in the former list (2099.) were made positive, there was not one exception.

2123. The remarkable power of oil, oil of turpentine, resin, &c., when in very small quantity, to change the exciting power of water, though as regards some of them (2112.) they are inactive without it, will excuse a few theoretical observations upon their mode of action. In the first place it appears that steam alone cannot by friction excite the electricity, but that the minute globules of water which it carries with it being swept over, rubbed upon and torn from the rubbed body (2085.) excite it and are excited, just as when the hand is passed over a rod of shell-lac. When olive oil or oil of turpentine is present, these globules are, I believe, virtually converted into globules of these bodies, and it is no longer water, but the new fluids which are rubbing the rubbed bodies.

2124. The reasons for this view are the following. If a splinter of wood dipped in olive oil or oil of turpentine touch the surface of water, a pellicle of the former instantly darts and spreads over the surface of the latter. Hence it is pretty certain that every globule of water passing through the box C, containing olive oil or oil of turpentine, will have a pellicle over it. Again, if a metal, wooden, or other balance-pan be *well cleaned* and *wetted* with water, and then put on the surface of clean water in a dish, and the other pan be loaded until almost, but not quite able to pull the first pan from the water, it will give a rough measure of the cohesive force of the water. If now the oily splinter of wood touch any part of the clean surface of the water in the dish, not only will itspread over the whole surface, but cause the pan to separate from the water, and if the pan be put down again, the water in the dish will no longer be able to retain it. Hence it is evident that the oil facilitates the separation of the water into parts by a mechanical force not otherwise sufficient, and invests these parts with a film of its own substance.

2125. All this must take place to a great extent in the steam passage: the particles of water there must be covered each with a film of oil. The tenuity of this film is no objection to the supposition, for the action of excitement is without doubt at that surface where the film is believed to exist, and such a globule, though almost entirely water, may well act as an oil globule, and by its friction render the wood, &c. positive, itself becoming negative.

2126. That water which is rendered ineffective by a little saline or acid matter should still be able to show the effect of the film of oil (2121.) attached to it, is perfectly consistent with this view. So also is the still more striking fact that alkalized water (2092.) having no power of itself should deeply injure the power of olive oil or resin, and hardly touch that of oil of turpentine (2121.), for the olive oil or resin would no longer form a film over it but dissolve in it, on the contrary the oil of turpentine would form its film.

2127. That resin should produce a strong effect and sulphur not is also satisfactory, for I find resin in boiling hot water melts, and has the same effect on the balance (2124.) as oil, though more slowly; but sulphur has not this power, its point of fusion being too high.

2128. It is very probable that when wood, glass or even metal is rubbed by these oily currents, the oil may be considered as rubbing not merely against wood, &c., but water also, the water being now on the side of the thing rubbed. Under the circumstances water has much more attraction for the wood rubbed than oil has, for in the steam-current, canvas, wood, &c. which has been well soaked in oil for a long time are quickly dispossessed of it, and found saturated with water. In such case the effect would still be to increase the positive state of the substance rubbed, and the negative state of the issuing stream.

2129. Having carried the experiments thus far with steam, and having been led to consider the steam as ineffectual by itself, and merely the mechanical agent by which the rubbing particles were driven onwards, I proceeded to experiment with compressed air[1]. For this purpose I used a strong copper box of the capacity of forty-six cubic inches, having two stop-cocks, by one of which the air was always forced in, and the other retained for the exit aperture. The box was very carefully cleaned out by caustic potash. Extreme care was taken (and required) to remove and avoid oil, wax, or resin about the exit apertures. The air was forced into it by a condensing syringe, and in certain cases when I required dry air, four or five ounces of cylinder potassa fusa were put into the box, and the condensed air left in contact with the substance ten or fifteen minutes. The average quantity of air which issued and was used in each blast was 150 cubic inches. It was very difficult to deprive this air of the smell of oil which it acquired in being pumped through the condensing syringe.

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[1] Mr. Armstrong has also employed air in much larger quantities. Philosophical Magazine, 1841, vol. xviii. pp. 133, 328.

2130. I will speak first of undried common air: when such compressed air was let suddenly out against the brass or the wood cone (2077.), it rendered the cone negative, exactly as the steam and water had done (2097.). This I attributed to the particles of water suddenly condensed from the expanding and cooled air rubbing against the metal or wood: such particles were very visible in the mist that appeared, and also by their effect of moistening the surface of the wood and metal. The electricity here excited is quite consistent with that evolved by steam and water: but the idea of that being due to evaporation (2083.) is in striking contrast with the actual condensation here.

2131. When however common air was let out against ice it rendered the ice *positive*, again and again, and that in alternation with the negative effect upon wood and metal. This is strongly in accordance with the high positive position which has already been assigned to water (2107.).

2132. I proceeded to experiment with dry air (2129.), and found that it was in all cases quite *incapable* of exciting electricity against wood or sulphur, or brass, in the form of cones (2077. 2097.); yet if, in the midst of these experiments, I let out a portion of air immediately after its compression, allowing it no time to dry, then it rendered the rubbed wood or brass negative (2130.). This is to me a satisfactory proof that in the former case the effect was due to the condensed water, and that neither *air alone* nor *steam alone* can excite these bodies, wood, brass, &c., so as to produce the effect now under investigation.

2133. In the next place the box C was attached to this air apparatus and experiments made with different substances introduced into it (2108.), using common air as the carrying vehicle.

2134. With distilled water in C, the metal cone was every now and then rendered negative, but more frequently no effect was produced. The want of a continuous jet of air sadly interfered with the proper adjustment of the proportion of water to the issuing stream.

2135. With common water (2090.), or a very dilute saline solution, or very dilute sulphuric acid (2091.) or ammonia, I never could obtain any traces of electricity.

2136. With oil of turpentine only in box C, the metal cone was rendered positive; but when both distilled water and oil of turpentine were introduced, the cone was very *positive*, indeed far more so than before. When sent against ice, the ice was made positive.

2137. In the same manner olive oil and water in C, or resin in alcohol and water in C, rendered the cone positive, exactly as if these substances had been carried forward in their course by steam.

2138. Although the investigation as respects the steam stream may here be considered as finished, I was induced in connection with the subject to try a few experiments with the air current and dry powders. *Sulphur* in powder (sublimed) rendered both metal and wood, and even the sulphur cone negative, only once did it render metal positive. *Powdered resin* generally rendered metal negative, and wood positive, but presented irregularities, and often gave *two states in the same experiment*, first diverging the electrometer leaves, and yet at the end leaving them uncharged. *Gum* gave unsteady and double results like the resin. *Starch* made wood negative. *Silica*, being either very finely powdered rock-crystal or that precipitated from fluo-silicic acid by water, gave very constant and powerful results, but both metal and wood were made strongly positive by it, and the silica when caught on a wet insulated board and examined was found to be negative.

2139. These experiments with powders give rise to two or three observations. In the first place the high degree of friction occurring between particles carried forward by steam or air was well illustrated by what happened with sulphur; it was found driven into the dry box-wood cone opposed to it with such force that it could not be washed or wiped away, but had to be removed by scraping. In the next place, the *double* excitements were very remarkable. In a single experiment, the gold leaves would open out very wide at first, and then in an instant as suddenly fall, whilst the jet still continued, and remains at last either neutral or a very little positive or negative: this was particularly the case with gum and resin. The fixation upon the wood of some of the particles issuing at the begining of the blast and the condensation of moisture by the expanding air, are circumstances which, with others present, tend to cause these variable results.

2140. Sulphur is nearly constant in its results, and silica very constant, yet their states are the reverse of those that might have been expected. Sulphur in the lump is rendered negative whether rubbed against wood or any of the metals which I have tried, and renders them *positive* (2141.), yet in the above experiments it almost always made both negative. Silica, in the form of a crystal, by friction with wood and metals renders them *negative*, but applied as above, it constantly made them strongly positive. There must be some natural cause for these changes, which at present can only be considered as imperfect results, for I have not had time to investigate the subject.

2141. In illustration of the effect produced by steam and water striking against other bodies, I rubbed these other substances (2099.) together in pairs to ascertain their order, which was as follows:—

1. Catskin or bearskin.
2. Flannel.
3. Ivory.
4. Quill.
5. Rock-crystal.
6. Flint glass.
7. Cotton.
8. Linen, canvas.
9. White silk.
10. The hand.
11. Wood.
12. Lac.
13. Metals . . .
    - Iron.
    - Copper.
    - Brass.
    - Tin.
    - Silver.
    - Platinum.
14. Sulphur

Any one of these became negative with the substances above, and positive with those beneath it. There are however many exceptions to this general statement: thus one part of a catskin is very negative to another part, and even to rock-crystal: different pieces of flannel also differ very much from each other.

2142. The mode of rubbing also makes in some cases a great difference, although it is not easy to say why, since the particles that actually rub ought to present the same constant difference; a feather struck lightly against dry canvas will become strongly negative, and yet the same feather drawn with a little pressure between the folds of the same canvas will be strongly positive, and these effects alternate, so that it is easy to take away the one state in a moment by the degree of friction which produces the other state. When a piece of flannel is halved and the two pieces drawn across each other, the two pieces will have different states irregularly, or the same piece will have both states in different parts, or sometimes both pieces will be negative, in which case, doubtless, air must have been rendered positive, and then dissipated.

2143. Ivory is remarkable in its condition. It is very difficult of excitement by friction with the metals, much more so than linen, cotton, wood, &c., which are lower in the scale than it (2141.), and withal are much better conductors, yet both circumstances would have led to the expectation that it would excite better than them when rubbed with metals. This property is probably very influential in giving character to it as a non-exciting steam passage (2102.).

2144. Before concluding this paper, I will mention, that having used a thin ivory tube fixed in a cork (2076.) for many experiments with oil, resin, &c., it at last took up such a state as to give not merely a non-exciting passage for the steam, but to exert upon it a nullifying effect, for the jet of steam and water passing through it produced no excitation against any of the bodies opposed, as on the former occasion, to it (2099.). The tube was apparently quite clean, and was afterwards soaked in alcohol to remove any resin, but it retained this peculiar state.

2145. Finally, I may say that the cause of the evolution of electricity by the liberation of confined steam is not evaporation; and further, being, I believe, friction, it has no effect in producing, and is not connected with, the general electricity of the atmosphere: also, that as far as I have been able to proceed, pure gases, *i. e.* gases not mingled with solid or liquid particles, do not excite electricity by friction against solid or liquid substances[1].

PLATE I.

Description of the Apparatus represented in section, and to a scale of one-fourth.

Fig. 1. The steam-globe (2076.), principal steam-cock, and drainage-cock to remove the water condensed in the pipe. The current of steam, &c. travelled in the direction of the arrow-heads.

Fig. 2. The cone apparatus (2077.) in one of its forms. The cone could be advanced and withdrawn by means of the milled head and screw.

Fig. 3. The feeding apparatus (2078.). The feeder was a glass tube or retort neck fitted by a cork into the cap of the feeding stop-cock. Other apparatus, as that figured 2, 5, 6, could be attached by a connecting piece to this apparatus.

Fig. 4. The chamber C (2079.) fitted by a cork on to a metal pipe previously screwed into the steam-globe; and having a metallic tube and adjusting piece screwed into its mouth. Other parts, as the cone fig. 2, or the wooden or glass tubes 5, 6, could be conjoined with this chamber.

Fig. 5. The box-wood tube (2102.).

Fig. 6. A glass or thin metal tube (2076.) attached by a cork to a mouth-piece fitting into the steam-globe.

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[1] References to papers in the Philosophical Magazine, 1840–1843. Armstrong, Phil. Mag. vol. xvii. pp. 370, 452; vol. xviii. pp. 50, 133, 328; vol. xix. p. 25; vol. xx. p. 5; vol. xxii. p. 1. Pattinson, Phil. Mag. vol. xvii. pp. 375, 457. Schafhaeutl, Phil. Mag. vol. xvii. p. 449; vol. xviii. pp. 14, 95, 265. See also Philosophical Magazine, 1843, xxiii. p. 194, for Armstrong's account of the *Hydro-electric Machine.*

PAPERS ON ELECTRICITY FROM THE QUARTERLY JOURNAL OF SCIENCE, PHILOSOPHICAL MAGAZINE, &c.

On some new Electro-Magnetical Motions, and on the Theory of Magnetism[1].

IN making an experiment the beginning of last week, to ascertain the position of the magnetic needle to the connecting wire of a voltaic apparatus, I was led into a series which appear to me to give some new views of electro-magnetic action, and of magnetism altogether; and to render more distinct and clear those already taken. After the great men who have already experimented on the subject, I should have felt doubtful that anything I could do could be new or possess an interest, but that the experiments seem to me to reconcile considerably the opposite opinions that are entertained on it. I am induced in consequence to publish this account of them, in the hope they will assist in making this important branch of knowledge more perfect.

The apparatus used was that invented by Dr. Hare of Philadelphia, and called by him a calorimotor; it is in fact a single pair of large plates, each having its power heightened by the induction of others, consequently all the positions and motions of the needles, poles, &c., are opposite to those produced by an apparatus of several plates; for, if a current be supposed to exist in the connecting wire of a battery from the zinc to the copper, it will be in each connected pair of plates from the copper to the zinc; and the wire I have used is that connection between the two plates of one pair. In the diagrams I may have occasion to subjoin, the ends of a connecting wire, marked Z and C, are connected with the zinc and copper-plates respectively; the sections are all horizontal and seen from above, and the arrow-heads have been used sometimes to mark the pole of a needle or magnet which points to the north, and sometimes to mark the direction of motion; no difficulty can occur in ascertaining to which of those uses any particular head is applied.

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[1] Quarterly Journal of Science, xii. 74.

On placing the wire perpendicularly, and bringing a needle towards it to ascertain the attractive and repulsive positions with regard to the wire; instead of finding these to be four, one attractive and one repulsive for each pole, I found them to be eight, two attractive and two repulsive for each pole; thus allowing the needle to take its natural position across the wire, which is exactly opposite to that pointed out by Œrsted for the reason before mentioned, and then drawing the support away from the wire slowly, so as to bring the north pole, for instance, nearer to it, there is attraction, as is to be expected; but on continuing to make the end of the needle come nearer to the wire, repulsion takes place, though the wire still be on the same side of the needle. If the wire be on the other side of the same pole of the needle, it will repel it when opposite to most parts between the centre of motion and the end; but there is a small portion at the end where it attracts it. Fig. 1, plate II., shows the positions of attraction for the north and south poles, fig. 2 the positions of repulsion.

If the wire be made to approach perpendicularly towards one pole of the needle, the pole will pass off on one side, in that direction which the attraction and repulsion at the extreme point of the pole would give; but, if the wire be continually made to approach the centre of motion, by either the one or other side of the needle, the tendency to move in the former direction diminishes; it then becomes null, and the needle is quite indifferent to the wire, and ultimately the motion is reversed, and the needle powerfully endeavours to pass the opposite way.

It is evident from this that the centre of the active portion of either limb of the needle, or the true pole, as it may be called, is not at the extremity of the needle, but may be represented by a point generally in the axis of the needle, at some little distance from the end. It was evident, also, that this point had a tendency to revolve round the wire, and necessarily, therefore, the wire round the point; and as the same effects in the opposite direction took place with the other pole, it was evident that each pole had the power of acting on the wire by itself, and not as any part of the needle, or as connected with the opposite pole.

By attending to fig. 3, which represents sections of the wire in its different positions to the needle, all this will be plain; the active poles are represented by two dots, and the arrow-heads show the tendency of the wire in its positions to go round these poles.

Several important conclusions flow from these facts; such as that there is no attraction between the wire and either pole of a magnet; that the wire ought to revolve round a magnetic pole and a magnetic pole round the wire; that both attraction and repulsion of connecting wires, and probably magnets, are compound actions; that true magnetic poles are centres of action induced by the whole bar, &c. &c. Such of these as I have been able to confirm by experiment, shall be stated, with their proofs.

The revolution of the wire and the pole round each other being the first important thing required to prove the nature of the force mutually exerted by them, various means were tried to succeed in producing it. The difficulty consisted in making a suspension of part of the wire sufficiently delicate for the motion, and yet affording sufficient mass of matter for contact. This was overcome in the following manner:—A piece of brass wire had a small button of silver soldered on to its end, a little cup was hollowed in the silver, and the metal being amalgamated, it would then retain a drop of mercury in it, though placed upside down for an upper centre of motion; for a lower centre, a similar cup was made of copper, into which a little mercury was put; this was placed in a jar of water under the former centre. A piece of copper wire was then bent into the form of a crank, its ends amalgamated, and the distances being arranged, they were placed in the cups. To prevent too much friction from the weight of the wire on the lower cup, it had been passed through a cork duly adjusted in size, and that being pushed down on the wire till immersed in the water, the friction became very little, and the wire very mobile, yet with good contacts. The plates being then connected with the two cups, the apparatus was completed. In this state, a magnetic pole being brought to the centre of motion of the crank, the wire immediately made an effort to revolve until it struck the magnet, and that being rapidly brought round to the other side, the wire again made a revolution, giving evidence that it would have gone round continually but for the extension of the magnet on the outside. To do away with this impediment, the wire and lower metal cup were removed, and a deep basin of mercury placed beneath; at the bottom of this was a piece of wax, and a small round bar magnet was stuck upright in it, so that one pole was about half or three-fourths of an inch above the surface of the mercury, and directly under the silver cup. A straight piece of copper wire, long enough to reach from the cup, and dip about half an inch into the mercury, had its ends amalgamated, and a small round piece of cork fixed on to one of them to make it more buoyant; this being dipped in the mercury close beside the magnet, and the other end placed under the little cup, the wire remained upright, for the adhesion of the cork to the magnet was sufficient for that purpose, and yet at its lower end had freedom of motion round the pole. The connection being now made from the plates to the upper cup, and to the mercury below, the wire immediately began to revolve round the pole of the magnet, and continued to do so as long as the connexion was continued.

When it was wished to give a large diameter to the circle described by the wire, the cork was moved from the magnet, and a little loop of platinum passed round the magnet and wire, to prevent them from separating too far. Revolution again took place on making the connexion, but more slowly as the distance increased.

The direction in which the wire moved was according to the way in which the connexions were made, and to the magnetic pole brought into action. When the upper part of the wire was connected with the zinc, and the lower with the copper plate, the motion round the north and south poles of a magnet were as in figs. 4 and 5, looking from above; when the connexions were reversed, the motions were in the opposite direction.

On bringing the magnetic pole from the centre of motion to the side of the wire, there was neither attraction nor repulsion; but the wire endeavoured to pass off in a circle, still having the pole for its centre, and that either to the one side or the other, according to the above law.

When the pole was on the outside of the wire, the wire moved in a direction directly contrary to that taken when the pole was in the inside; but it did not move far, the endeavour was still to go round the pole as a centre, and it only moved till that power and the power which retained it in a circle about its own axis were equipoised.

The next object was to make the magnet revolve round the wire. This was done by so loading one pole of the small magnet with platinum that the magnet would float upright in a basin of mercury, with the other pole above its surface; then connecting the mercury with one plate and bringing a wire from the other perpendicularly into it in another part near the floating magnet; the upper pole immediately began to revolve round the wire, whilst the lower pole being removed away caused no interference or counteracting effect.

The motions were again according to the pole and the connexions. When the upper part of the wire was in contact with the zinc plate, and the lower with the copper, the direction of the curve described by the north and south poles were as in figs. 6 and 7. When the connexions were reversed, the motions were in the opposite directions.

Having succeeded thus far, I endeavoured to make a wire and a magnet revolve on their own axis by preventing the rotation in a circle round them, but have not been able to get the slightest indications that such can be the case; nor does it, on consideration, appear probable. The motions evidently belong to the current, or whatever else it be, that is passing through the wire, and not to the wire itself, except as the vehicle of the current. When that current is made a curve by the form of the wire, it is easy to conceive how, in revolving, it should take the wire with it; but when the wire is straight, the current may revolve without any motion being communicated to the wire through which it passes.

M. Ampère has shown that two similar connecting wires, by which is meant, having currents in the same direction through them, attract each other, and that two wires having currents in opposite directions through them, repel each other, the attraction and repulsion taking place in right lines between them. From the attraction of the north pole of a needle on one side the wire, and of the south on the other, and the repulsion of the poles on the opposite sides, Dr. Wollaston called this magnetism vertiginous, and conceived that the phenomena might be explained upon the supposition of an electro-magnetic current passing round the axis of the conjunctive wire, its direction depending upon that of the electric current, and exhibiting north and south powers on the opposite sides. It is, indeed, an ascertained fact, that the connecting wire has different powers at its opposite sides; or rather, each power continues all round the wire, the direction being the same, and hence it is evident that the attractions and repulsions of M. Ampère's wires are not simple, but complicated results.

A simple case which may be taken of magnetic motion, is the circle described by the wire or the pole round each other. If a wire be made into a helix, as M. Ampère describes, the arrangement is such that all the vertiginous magnetism, as Dr. Wollaston has named it, of the one kind, or one side of the wire, is concentrated in the axis of the helix, whilst the contrary kind is very much diffused, *i. e.* the power exerted by a great length of wire to make a pole pass one way round it, all tends to carry that pole to a particular spot, whilst the opposite power is diffused and much weakened in its action on any one pole. Hence the power on one side of the wire is very much concentrated, and its particular effects brought out strongly, whilst that on the other is rendered insensible. A means is thus obtained of separating, as it were, the one power from the other; but when this is done, and we examine the end of the helix, it is found very much to resemble a magnetic pole; the power is concentrated at the extremity of the helix; it attracts or repels one pole in all directions; and I find that it causes the revolution of the connecting wire round it, just as a magnetic pole does. Hence it may, for the present, be considered identical with a magnetic pole; and I think that the experimental evidence of the ensuing pages will much strengthen that opinion.

Assuming, then, that the pole of a magnetic needle presents us with the properties of one side of the wire, the phenomena it presents with the wire itself, offers us a means of analysis, which, probably, if well pursued, will give us a much more intimate knowledge of the state of the powers active in magnets. When it is placed near the wire, always assuming the latter to be connected with the battery, it is made to revolve round it, passing towards that side by which it is attracted, and from that side by which it is repelled, *i. e.* the pole is at once attracted and repelled by equal powers, and therefore neither recedes nor approaches; but the powers being from opposite sides of the wire, the pole in its double effort to recede from one side and approach the other revolves in the circle, that circle being evidently decided by the particular pole and state of the wire, and deducible from the law before mentioned.

The phenomena presented by the approximation of one pole to two or more wires, or two poles to one or more wires, offer many illustrations of this double action, and will lead to more correct views of the magnet. These experiments are easily made by loading a needle with platinum at one pole, that the other may float above mercury, or by almost floating a small magnetic needle by cork in a basin of water, at the bottom of which is some mercury with which to connect the wires. In describing them I shall refrain from entering into all their variations, or pursuing them to such conclusions as are not directly important.

Two similar wires, Ampère has shown, attract each other; and Sir H. Davy has shown that the filings adhering to them attract from one to another on the same side. They are in that position in which the north and south influence of the different wires attract each other. They seem also to neutralize each other in the parts that face, for the magnetic pole is quite inactive between them, but if put close together, it moves round the outside of both, circulating round them as round one wire, and their influences being in the same direction, the greatest effect is found to be at the further outside surfaces of the wires. If several similar wires be put together, side by side like a ribbon, the result is the same, and the needle revolves round them all; the internal wires appear to lose part of their force, which is carried on towards the extreme wire in opposite directions, so that the floating pole is accelerated in its motion as it passes by the edges that they form. If, in place of a ribbon of parallel wires, a slip of metal be used, the effect is the same, and the edges act as if they contained in a concentrated state the power that belonged to the inner portion of the slip. In this way we procure the means of removing, as it were, in that direction, the two sides of the wire from each other.

If two wires in opposite states be arranged parallel to each other, and the pole be brought near them, it will circulate round either of them in obedience to the law laid down; but as the wires have opposite currents, it moves in opposite directions round the two, so that when equidistant from them, the pole is propelled in a right line perpendicular to the line which joins them, either receding or approaching; and if it approaches, passing between and then receding; hence it exhibits the curious appearance of first being attracted by the two wires, and afterwards repelled (fig. 8.). If the connexion with both wires be inverted, or if the pole be changed, the line it describes is in the opposite direction. If these two opposite currents be made by bending a piece of silked wire parallel to itself, fig. 9, it, when connected with the apparatus, becomes a curious magnet; with the north pole, for instance, it attracts powerfully on one side at the line between the two currents, but repels strongly to the right or left; whilst on the other side the line repels the north pole, but attracts it strongly to the right or left. With the south pole the attractions and repulsions are reversed.

When both poles of the needle were allowed to come into action on the wire or wires, the effects were in accordance with those described. When a magnetic needle was floated on water, and the perpendicular wire brought towards it, the needle turned round more or less, until it took a direction perpendicular to, and across the wire, the poles being in such positions that either of them alone would revolve round the wire in a circle proceeding by the side to which it had gone, according to the law before stated. The needle then approaches to the wire, its centre (not either pole) going in a direct line towards it. If the wire be then lifted up and put down the other side of the needle, the needle passes on in the same line receding from the wire, so that the wire seems here to be both attractive and repulsive of the needle. This effect will be readily understood from fig. 10, where the poles and direction of the wire are not marked, because they are the same as before. If either be reversed, the others reverse themselves. The experiment is analogous to the one described above; there the pole passed between two dissimilar wires, here the wire between two dissimilar poles.

If two dissimilar wires be used, and the magnet have both poles active, it is repelled, turned round, or is attracted in various ways, until it settles across between the two wires; all its motions being easily reducible to those impressed on the poles by the wires, both wires and both poles being active in giving that position. Then if it happens not to be midway between the two, or they are not of equal power, it goes slowly towards one of them, and acts with it just as the single wire of the last paragraph.

Figs. 11 and 12 exhibit more distinctly the direction of the forces which influence the poles in passing between two dissimilar wires: fig. 11, when the pole draws up between the wires; fig. 12, the pole thrown out from between them. The poles and state of the wire are not marked, because the diagrams illustrate the attraction and repulsion of both poles; for any particular pole, the connexion of the wires must be accordingly.

If one of the poles be brought purposely near either wire in the position in which it appears to attract most strongly, still if freedom of motion be given by a little tapping, the needle will slip along till it stands midway across the wire.

A beautiful little apparatus has been made by M. de la Rive to whom I am indebted for one of them, consisting of a small voltaic combination floating by a cork; the ends of the little zinc and copper slips come through the cork, and are connected above by a piece of silked wire which has been wrapped four or five times round a cylinder, and the wires tied together with a silk thread so as to form a close helix about one inch in diameter. When placed on acidulated water it is very obedient to the magnet and serves admirably to transform, as it were, the experiments with straight wires that have been mentioned, to the similar ones made with helices. Thus, if a magnet be brought near it and level with its axis, the apparatus will recede or turn round until that side of the curve next to the nearest pole is the side attracted by it. It will then approach the pole, pass it, recede from it until it gains the middle of the magnet, where it will rest like an equator round it, its motions and position being still the same as those before pointed out (fig. 13.). If brought near either pole it will still return to the centre; and if purposely placed in the opposite direction at the centre of the magnet, it will pass off by either pole to which it happens to be nearest, being apparently first attracted by the pole and afterwards repelled, as is actually the case; will, if any circumstance disturbs its perpendicularity to the magnet, turn half way round; and will then pass on to the magnet again, into the position first described. If, instead of passing the magnet through the curve, it be held over it, it stands in a plane perpendicular to the magnet, but in an opposite direction to the former one. So that a magnet, both within and without this curve, causes it to direct.

When the poles of the magnet are brought over this floating curve, there are some movements and positions which at first appear anomalous, but are by a little attention easily reducible to the circular movement of the wire about the pole. I do not think it necessary to state them particularly.

The attractive and repulsive positions of this curve may be seen by fig. 13, the curve in the two dotted positions is attracted by the poles near them. If the positions be reversed, repulsion takes place.

From the central situation of the magnet in these experiments, it may be concluded that a strong and powerful curve or helix would suspend a powerful needle in its centre. By making a needle almost float on water and putting the helix over a glass tube, this result has in part been obtained.

In all these magnetic movements between wires and poles, those which resemble attraction and repulsion, that is to say, those which took place in right lines, required at least either two poles and a wire, or two wires and a pole; for such as appear to exist between the wire and either pole of the battery, are deceptive and may be resolved into the circular motion. It has been allowed, I believe, by all who have experimented on these phenomena, that the similar powers repel and the dissimilar powers attract each other; and that, whether they exist in the poles of the magnets or in the opposite sides of conducting wires. This being admitted, the simplest cases of magnetic action will be those exerted by the poles of helices, for, as they offer the magnetic states of the opposite sides of the wire independent, or nearly so, one of the other, we are enabled by them to bring into action two of those powers only, to the exclusion of the rest; and, from experiment it appears that when the powers are similar, repulsion takes place, and when dissimilar, attraction; so that two cases of repulsion and one of attraction are produced by the combination of these magnetic powers[1].

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[1] This is perhaps not strictly true, because, though the opposite powers are weakened, they still remain in action.

The next cases of magnetic motion, in the order of simplicity, are those where three powers are concerned, or those produced by a pole and a wire. These are the circular motions described in the early part of this paper. They resolve themselves into two; a north pole and the wire round each other, and a south pole and the wire round each other. The law which governs these motions has been stated.

Then follow the actions between two wires: these when similarly electrified attract as M. Ampère has shown; for then the opposite sides are towards each other, and the four powers all combine to draw the currents together, forming a double attraction; but when the wires are dissimilar they repel, because, then on both sides of the wire the same powers are opposed, and cause a double repulsion.

The motions that result from the action of two dissimilar poles and a wire next follow: the wire endeavours to describe opposite circles round the poles; consequently it is carried in a line passing through the central part of the needle in which they are situated. If the wire is on the side on which the circles close together, it is attracted; if on the opposite side, from whence the circles open, it is repelled, fig. 10.

The motions of a pole with two wires are almost the same as the last; when the wires are dissimilar, the pole endeavours to form two opposite circles about the wires; when it is on that side of the wires on which the circles meet, it is attracted; when on the side on which they open, it is repelled, figs. 8, 11, 12.

Finally, the motion between two poles and two dissimilar wires, is an instance where several powers combine to produce an effect.

M. Ampère, whilst reasoning on the discovery of M. Oersted, was led to the adoption of a theory, by which he endeavoured to account for the properties of magnets, by the existence of concentric currents of electricity in them, arranged round the axis of the magnet. In support of this theory, he first formed the spiral or helix wire, in which currents could be made to pass nearly perpendicular to, and round the axis of a cylinder. The ends of such helices were found when connected with the voltaic apparatus to be in opposite magnetic states, and to present the appearance of poles. Whilst pursuing the mutual action of poles and wires, and tracing out the circular movements, it seemed to me that much information respecting the competency of this theory might be gained from an attempt to trace the action of the helix, and compare it with that of the magnet more rigorously than had yet been done; and to form artificial electro-magnets, and analyse natural ones. In doing this, I think I have so far succeeded as to trace the action of an electro-magnetic pole, either in attracting or repelling, to the circulating motion before described.

If three inches of connecting wire be taken, and a magnetic pole be allowed to circulate round the middle of it, describing a circle of a little less than one inch in diameter, it will be moved with equal force in all parts of the circle, fig. 14; bend then the wire into a circle, leaving that part round which the pole revolves perpendicularly undisturbed, as seen by the dotted lines, and make it a condition that the pole be restrained from moving out of the circle by a radius. It will immediately be evident that the wire now acts very differently on the pole in the different parts of the circle it describes. Every part of it will be active at the same time on the pole, to make it move through the centre of the wire ring, whilst as it passes away from that position the powers diverge from it, and it is either removed from their action or submitted to opposing ones, until on its arriving at the opposite part of the circle it is urged by a very small portion indeed of those which moved it before. As it continues to go round, its motion is accelerated, the forces rapidly gather together on it, until it again reaches the centre of the wire ring where they are at their highest, and afterwards diminish as before. Thus the pole is perpetually urged in a circle, but with powers constantly changing.

If the wire ring be conceived to be occupied by a plane, then the centre of that plane is the spot where the powers are most active on the pole, and move it with most force. Now this spot is actually the pole of this magnetic apparatus. It seems to have powers over the circulating pole, making it approach or attracting it on the one side, and making it recede or repelling it on the other, with powers varying as the distance; but its powers are only apparent, for the force is in the ring, and this spot is merely the place where they are most accumulated; and though it seems to have opposite powers, namely, those of attracting and repelling; yet this is merely a consequence of its situation in the circle, the motion being uniform in its direction, and really and truly impressed on the pole by its motor, the wire.

At page 133 it was shown that two or more similar wires put together in a line, acted as one; the power being, as it were, accumulated towards the extreme wires, by a species of induction taking place among them all; and at the same time was noticed the similar case of a plate of metal connecting the ends of the apparatus, its powers being apparently strongest at the edges. If, then, a series of concentric rings be placed one inside the other, they having the electric current sent through them in the same direction; or if, which is the same thing, a flat spiral of silked wire passing from the centre to the circumference be formed, and its ends be in connexion with the battery, fig. 15, then the circle of revolution would still be as in fig. 14, passing through the centre of the rings or spiral, but the power would be very much increased. Such a spiral, when made, beautifully illustrates this fact; it takes up an enormous quantity of iron filings, which approach to the form of cones, so strong is the action at the centre; and its action on the needle by the different sides, is eminently powerful.

If in place of putting ring within ring, they be placed side by side, so as to form a cylinder, or if a helix be made, then the same kind of neutralization takes place in the intermediate wires, and accumulated effect in the extreme ones, as before. The line which the pole would now travel, supposing the inner end of the radius to move over the inner and outer surface of the cylinder, would be through the axis of the cylinder round the edge to one side, back up that side, and round to the axis, down which it would go, as before. In this case the force would probably be greatest at the two extremes of the axis of the cylinder, and least at the middle distance on the outside.

Now consider the internal space of the cylinder filled up by rings or spirals, all having the currents in the same direction; the direction and kind of force would be the same, but very much strengthened: it would exist in the strongest degree down the axis of the mass, because of the circular form, and it would have the two sides of the point in the centre of the simple ring, which *seemed* to possess attractive and repulsive powers on the pole, removed to the ends of the cylinder; giving rise to two points, apparently distinct in their action, one being attractive, and the other repulsive, of the poles of a magnet. Now conceive that the pole is not confined to a motion about the sides of the ring, or the flat spiral, or cylinder; it is evident that if placed in the axis of any of them at a proper distance for action, it, being impelled by two or more powers in equal circles, would move in a right line in the intersection of those circles, and approach directly to or recede from, the points before spoken of, giving the appearance of a direct attraction and repulsion; and if placed out of that axis, it would move towards or from the same spot in a curve line, its direction and force being determined by the curve lines representing the active forces from the portions of wire forming the ends of the cylinder, spiral, or ring, and the strength of those forces.

Thus the phenomena of a helix, or a solid cylinder of spiral silked wire, are reduced to the simple revolution of the magnetic pole round the connecting wire of the battery, and its resemblance to a magnet is so great, that the strongest presumption arises in the mind, that they both owe their powers, as M. Ampère has stated, to the same cause. Filings of iron sprinkled on paper held over this cylinder, arranged themselves in curved lines passing from one end to the other, showing the path the pole would follow, and so they do over a magnet; the ends attract and repel as do those of a magnet; and in almost every point do they agree. The following experiments will illustrate and confirm the truth of these remarks on the action of the ring, helix, or cylinder; and will show in what their actions agree with, and differ (for there are differences) from, the action of a magnet.

A small magnet being nearly floated in water by cork, a ring of silked copper wire, fig. 16, having its ends connected with the battery, was brought near its poles in different positions; sometimes the pole was repelled from, sometimes attracted into, the ring, according to the position of the pole, and the connexions with the battery. If the wire happened to be opposite to the pole, the pole passed sideways and outwards when it was repelled, and sideways and inwards when it was attracted; and on entering within the ring and passing through, it moved sideways in the opposite direction, endeavouring to go round the wire. The actions also presented by M. de la Rive's ring are actions of this kind, and indeed are those which best illustrate the relations between the ring and the pole; some of them have been mentioned, and if referred to, will be found to accord with the statement given.

With a flat spiral the magnetic power was very much increased; and when the rings were not continued to the centre, the power of the inner edge over the outer was well shown either by the pole of a needle, or iron filings. With the latter the appearance was extremely beautiful and instructive; when laid flat upon a heap of them, they arranged themselves in lines, passing through the ring parallel to its axis, and then folding up on either side as radii round to the edge, where they met; so that they represented, exactly, the lines which a pole would have described round the sides of the rings; and those filings which were in the axis of the rings, stood up in perpendicular filaments, half an inch long and so as to form an actual axis to the ring, tending neither one way nor the other, but according in their form and arrangement with what has been described; whilst the intermediate portion also formed long threads, bending this way and that from the centre, more or less, according as they were further from, or nearer to it.

With a helix the phenomena were interesting, because according to the view given of the attractions and repulsions, that is of the motions toward and from the ends, some conclusions should follow, that if found to be true in fact, and to hold also with magnets, would go far to prove the identity of the two. Thus the end which seems to attract a certain pole on the outside, ought to repel it as it were on the inside, and that which seems to repel it on the outside, ought to appear to attract it on the inside; *i. e.* that as the motions on the inside and outside are in different directions for the same pole, it would move in the one case to and in the other case from the same end of the helix. Some phenomena of this kind have been described in explaining figs. 8, 11, 12, and 13; others are as follows.

A helix of silked copper wire was made round a glass tube, the tube being about an inch in diameter; the helix was about three inches long. A magnetic needle nearly as long was floated with cork, so as to move about in water with the slightest impulse. The helix being connected with the apparatus and put into the water in which the needle lay, its ends appeared to attract and repel the poles of the needle according to the laws before mentioned. But, if that end which attracted one of the poles of the needle was brought near that pole, it entered the glass tube, but did not stop just within side in the neighbourhood of this pole (as we may call it for the moment) of the helix, but passed up the tube, drawing the whole needle in, and went to the opposite pole of the helix, or the one which on the outside would have repelled it; on trying the other pole of the magnet with its corresponding end or pole of the helix the same effect took place; the needle pole entered the tube and passed to the other end, taking the whole needle into the same position it was in before.

Thus each end of the helix seemed to attract and repel both poles of the needle; but this is only a natural consequence of the circulating motion before experimentally demonstrated, and each pole would have gone through the helix and round on the outside, but for the counteraction of the opposite pole. It has been stated that the poles circulate in opposite directions round the wires, and they would consequently circulate in opposite directions through and round the helix; when, therefore, one end of the helix was near that pole, which would, according to the law stated, enter it and endeavour to go through, it would enter, and it would continue its course until the other pole, at first at a distance, would be brought within action of the helix; and, when they were both equally within the helix and consequently equally acted on, their tendency to go in different directions would counterbalance each other, and the needle would remain motionless. If it were possible to separate the two poles from each other, they would dart out of each end of the helix, being apparently repelled by those parts that before seemed to attract them, as is evident from the first and many other experiments.

By reversing the needle and placing it purposely in the helix in that position, the poles of the needle and the corresponding poles of the helix as they attract on the outside, are brought together on the inside, but both pairs now seem to repel; and, whichever end of the helix the needle happens to be nearest to, it will be thrown out at. This motion may be seen to exhibit in its passing state, attraction between similar poles, since the inner and active pole is drawn towards that end on the inside, by which it is thrown off on the outside[1].

These experiments may be made with the single curve of M. de la Rive, in which case it is the wire that moves and not the magnet; but as the motions are reciprocal, they may be readily anticipated.

A plate of copper was bent nearly into a cylinder, and its edges made to dip into two portions of mercury; when placed in a current it acted exactly as a helix.

A solid cylinder of silked wire was made exactly in fashion like a helix, but that one length of the wire served as the axis, and the folds were repeated over and over again. This as well as the former helix, had poles the same in every respect as to kind as the north and south poles of a magnet; they took up filings, they made the connecting wire revolve, they attracted and repelled in four parallel positions as is described of common magnets in the first pages of this paper, and filings sprinkled on paper over them, formed curves from one to the other as with magnets; these lines indicating the direction in which a north or south pole would move about them.

Now with respect to the accordance which is found between the appearances of a helix or cylinder when in the voltaic circuit, and a cylindrical common magnet, or even a regular square bar magnet; it is so great, as at first to leave little doubt, that whatever it is that causes the properties of the one, also causes the properties of the other, for the one may be substituted for the other in, I believe, every magnetical experiment; and, in the bar magnet, all the effects on a single pole or filings, &c., agree with the notion of a circulation, which if the magnet were not solid would pass through its centre, and back on the outside.

The following, however, are differences between the appearances of a magnet and those of a helix or cylinder: one pole of a magnet attracts the opposite pole of a magnetic needle in all directions and positions; but when the helix is held along-side the needle nearly parallel to it, and with opposite poles together, so that attraction should take place, and then the helix is moved on so that the pole of the needle gradually comes nearer to the middle of the helix, repulsion generally takes place before the pole gets to the middle of the helix, and in a situation where with the magnet it would be attracted. This is probably occasioned by the want of continuity in the sides of the curves or elements of the helix, in consequence of which the unity of action which takes place in the rings into which a magnet may be considered to be divided is interfered with and disturbed.

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[1] The magnetizing power of the helix is so strong that if the experiment be made slowly, the needle will have its mangetism changed and the result will be fallacious.

Another difference is that the poles, or those spots to which the needle points when perpendicular to the ends or sides of a magnet or helix, and where the motive power may be considered perhaps as most concentrated, are in the helix at the extremity of its axis, and not any distance in from the end; whilst in the most regular magnets they are almost always situate in the axis at some distance in from the end; a needle pointing perpendicularly towards the end of a magnet is in a line with its axis, but perpendicularly to the side it points to a spot some distance from the end, whilst in the helix, or cylinder, it still points to the end. This variation is, probably, to be attributed to the distribution of the exciting cause of magnetism in the magnet and helix. In the latter, it is necessarily uniform everywhere, inasmuch as the current of electricity is uniform. In the magnet it is probably more active in the middle than elsewhere; for as the north pole of a magnet brought near a south one increases its activity, and that the more as it is nearer, it is fair to infer that the similar parts which are actually united in the inner part of the bar, have the same power. Thus a piece of soft iron put to one end of a horse-shoe magnet, immediately moves the pole towards that end; but if it be then made to touch the other end also, the pole moves in the opposite direction, and is weakened; and it moves the further, and is made weaker as the contact is more perfect. The presumption is, that if it were complete, the two poles of the magnet would be diffused over the whole of its mass, the instrument there exhibiting no attractive or repulsive powers. Hence it is not improbable that, caused by some induction, a greater accumulation of power may take place in the middle of the magnet than at the end, and may cause the poles to be inwards, rather than at the extremities.

A third difference is, that the similar poles of magnets, though they repel at most distances, yet when brought very near together, attract each other. This power is not strong, but I do not believe it is occasioned by the superior strength of one pole over the other, since the most equal magnets exert it, and since the poles as to their magnetism remain the same, and are able to take up as much, if not more, iron filings when together, as when separated, whereas opposite poles, when in contact, do not take up so much. With similar helix poles, this attraction does not take place.

The attempts to make magnets resembling the helix and the flat spirals, have been very unsuccessful. A plate of steel was formed into a cylinder and magnetized, one end was north all round, the other south; but the outside and the inside had the same properties, and no pole of a needle would have gone up the axis and down the sides, as with the helix, but would have stopped at the dissimilar pole of the needle. Hence it is certain, that the rings of which the cylinder may be supposed to be formed, are not in the same state as those of which the helix was composed. All attempts to magnetize a flat circular plate of steel, so as to have one pole in the centre of one side, and the other pole in the centre of the opposite side, for the purpose of imitating the flat spiral, fig. 15, failed; nothing but an irregular distribution of the magnetism could be obtained.

M. Ampère is, I believe, undecided with regard to the size of the currents of electricity that are assumed to exist in magnets, perpendicular to their axis. In one part of his memoirs they are said, I think, to be concentric, but this cannot be the case with those of the cylinder magnet, except two be supposed in opposite directions, the one on the inside, the other on the outside surface. In another part, I believe, the opinion is advanced that they may be exceedingly small; and it is, perhaps, possible to explain the cause of the most irregular magnet by theoretically bending such small currents in the direction required.

In the previous attempt to explain some of the electro-magnetic motions, and to show the relation between electro and other magnets, I have not intended to adopt any theory of the cause of magnetism, nor to oppose any. It appears very probable that in the regular bar magnet, the steel, or iron, is in the same state as the copper wire of the helix magnet; and perhaps, as M. Ampère supports in his theory, by the same means, namely, currents of electricity; but still other proofs are wanting of the presence of a power like electricity than the magnetic effects only. With regard to the opposite sides of the connecting wire, and the powers emanating from them, I have merely spoken of them as two, to distinguish the one set of effects from the other. The high authority of Dr. Wollaston is attached to the opinion that a single electro-magnetic current passing round the axis of the wire in a direction determined by the position of the voltaic poles, is sufficient to explain all the phenomena.

M. Ampère, who has been engaged so actively in this branch of natural philosophy, drew from his theory, the conclusion that a circular wire forming part of the connexion between the poles of the battery, should be directed by the earth's magnetism, and stand in a plane perpendicular to the magnetic meridian and the dipping needle. This result was said to be actually obtained, but its accuracy has been questioned, both on theoretical and experimental grounds. As the magnet directs the wire when in form of a curve, and the curve a needle, I endeavoured to repeat the experiment, and succeeded in the following manner. A voltaic combination of two plates was formed, which were connected by a copper wire, bent into a circular form; the plates were put into a small glass jar with dilute acid, and the jar floated on the surface of water; being then left to itself in a quiet atmosphere, the instrument so arranged itself that the curve was in a plane perpendicular to the magnetic meridian; when moved from this position, either one way or the other, it returned again; and on examining the side of the curve towards the north, it was found to be that, which, according to the law already stated, would be attracted by a south pole. A voltaic circle made in a silver capsule, and mounted with a curve, also produced the same effect; as did likewise, very readily, M. de la Rive's small ring apparatus[1]. When placed on acidulated water, the gas liberated from the plates prevented its taking up a steady position; but when put into a little floating cell, made out of the neck of a Florence flask, the whole readily took the position mentioned above, and even vibrated slowly about it.

As the straight connecting wire is directed by a magnet, there is every reason to believe that it will act in the same way with the earth, and take a direction perpendicular to the magnetic meridian. It also should act with the magnetic pole of the earth, as with the pole of a magnet, and endeavour to circulate round it. Theoretically, therefore, a horizontal wire perpendicular to the magnetic meridian, if connected first in one way with a voltaic battery, and then in the opposite way, should have its weight altered; for in the one case it would tend to pass in a circle downwards, and in the other upwards. This alteration should take place differently in different parts of the world. The effect is actually produced by the pole of a magnet, but I have not succeeded in obtaining it, employing only the polarity of the earth.—September 11, 1821.

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[1] Quarterly Journal of Science, xii. 186.

Electro-magnetic Rotation Apparatus[1].

Since the paper in the preceding pages has been printed, I have had an apparatus made by Mr. Newman, of Lisle-street, for the revolutions of the wire round the pole, and a pole round the wire. When Hare's calorimoter was connected with it, the wire revolved so rapidly round the pole, that the eye could scarcely follow the motion, and a single galvanic trough, containing ten pair of plates, of Dr. Wollaston's construction, had power enough to move the wire and the pole with considerable rapidity. It consists of a stand, about 3 inches by 6, from one end of which a brass pillar rises about 6 inches high, and is then continued horizontally by a copper-rod over the stand; at the other end of the stand a copper plate is fixed with a wire for communication, brought out to one side; in the middle is a similar plate and a wire; these are both fixed. A small shallow glass cup, supported on a hollow foot of glass has a plate of metal cemented to the bottom, so as to close the aperture and form a connexion with the plate on the stand; the hollow foot is a socket, into which a small cylindrical bar magnet can be placed, so that the upper pole shall be a little above the edge of the glass; mercury is then poured in until the glass is nearly full; a rod of metal descends from the horizontal arm perpendicularly over this cup; a little cavity is hollowed at the end and amalgamated, and a piece of stiff copper wire is also amalgamated, and placed in it as described in the paper, except that it is attached by a piece of thread in the manner of a ligament, passing from the end of the wire to the inner surface of the cup; the lower end of the wire is amalgamated, and furnished with a small roller, which dips so as to be under the surface of the mercury in the cup beneath it.

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[1] Quarterly Journal of Science, xii. 186.

The other plate on the stand has also its cup, which is nearly cylindrical, a metal pin passes through the bottom of it, to connect by contact with the plate below, and to the inner end of the pin a small round bar magnet is attached at one pole by thread, so as to allow the other to be above the surface of the mercury when the cup is filled, and have freedom of motion there; a thick wire descends from the rod above perpendicularly, so as to dip a little way into the mercury of the cup; it forms the connecting wire, and the pole can move in any direction round it. When the connexions are made with the pillar, and either of the wires from the stand plates, the revolution of the wire, or pole above, takes place; or if the wires be connected with the two coming from the plates, motion takes place in both cups at once, and in accordance with the law stated in the paper. This apparatus may be much reduced in size, and made very much more delicate and sensible.

Description of an Electro-Magnetical Apparatus for the Exhibition of Rotatory Motion[1].

The account given in the Miscellanea of the last Journal (p. 147.), of the apparatus invented in illustration of the paper in the body of that Number, being short and imperfect; a plate is given in the present Number, presenting a section of that apparatus, and a view of a smaller apparatus, illustrative of the motions of the wire and the pole round each other. The larger apparatus is delineated, fig. 1. Plate iv., on a scale of one half. It consists of two glass vessels, placed side by side with their appendages. In that on the left of the plate the motion of a magnetic pole round the connecting wire of the voltaic battery is produced. That a current of voltaic electricity may be established through this cup, a hole is drilled at the bottom, and into this a copper pin is ground tight, which projects upwards a little way into the cup, and below is riveted to a small round plate of copper, forming part of the foot of the vessel. A similar plate of copper is fixed to the turned wooden base on which the cup is intended to stand, and a piece of strong copper wire, which is attached to it beneath, after proceeding downwards a little way, turns horizontally to the left hand, and forms one of the connexions. The surfaces of these two plates intended to come together, are tinned and amalgamated, that they may remain longer clean and bright, and afford better contact. A small cylindrical and powerful magnet has one of its poles fastened to a piece of thread, which, at the other end, is attached to the copper pin at the bottom of the cup; and the height of the magnet and length of the thread is so adjusted, that when the cup is nearly filled with clean mercury, the free pole shall float almost upright on its surface.

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[1] Quarterly Journal of Science, xii. 283.

A small brass pillar rises from the stand behind the glass vessels: an arm comes forward from the top of it, supporting at its extremity a cross wire, which at the place on the left hand, where it is perpendicularly over the cup just described, bends downwards, and is continued till it just dips into the centre of the mercurial surface. The wire is diminished in size for a short distance above the surface of the mercury, and its lower extremity amalgamated, for the purpose of ensuring good contact; and so also is the copper pin at the bottom of the cup. When the poles of a voltaic apparatus are connected with the brass pillar, and with the lateral copper wire, the upper pole of the magnet immediately rotates round the wire which dips into the mercury; and in one direction or the other, according as the connexions are made.

The other vessel is of the form delineated in the plate. The stem is hollow and tubular; but, instead of being filled by a plug, as is the aperture in the first vessel, a small copper socket is placed in it, and retained there by being fastened to a circular plate below, which is cemented to the glass foot, so that no mercury shall pass out by it. This plate is tinned and amalgamated on its lower surface, and stands on another plate and wire, just as in the former instance. A small circular bar magnet is placed in the socket, at any convenient height, and then mercury poured in until it rises so high that nothing but the projecting pole of the magnet is left above its surface at the centre. The forms and relative positions of the magnet, socket, plate, &c., are seen in fig. 2.

The cross wire supported by the brass pillar is also prolonged on the right hand, until over the centre of the vessel just described; it then turns downwards and descends about half an inch: it has its lower extremity hollowed out into a cup, the inner surface of which is well amalgamated. A smaller piece of copper wire has a spherical head fixed on to it, of such a size that it may play in the cup in the manner of a ball and socket-joint, and being well amalgamated, it, when in the cup, retains sufficient fluid mercury by capillary attraction to form an excellent contact with freedom of motion. The ball is prevented from falling out of the socket by a piece of fine thread, which, being fastened to it at the top, passes through a small hole at the summit of the cup, and is made fast on the outside of the thick wire. This is more minutely explained by figs. 3 and 4. The small wire is of such a length that it may dip a little way into the mercury, and its lower end is amalgamated. When the connexions are so made with the pillar and right-hand wire, that the current of electricity shall pass through this moveable wire, it immediately revolves round the pole of the magnet, in a direction dependent on the pole used, and the manner in which the connexions are made.

Fig. 5 is the delineation of a small apparatus, the wire in which revolves rapidly, with very little voltaic power. It consists of a piece of glass tube, the bottom part of which is closed by a cork, through which a small piece of soft iron wire passes, so as to project above and below the cork. A little mercury is then poured in, to form a channel between the iron wire and the glass tube. The upper orifice is also closed by a cork, through which a piece of platinum wire passes which is terminated within by a loop; another piece of wire hangs from this by a loop, and its lower end, which dips a very little way into the mercury, being amalgamated, it is preserved from adhering either to the iron wire or the glass. When a very minute voltaic combination is connected with the upper and lower ends of this apparatus, and the pole of a magnet is placed in contact with the external end of the iron wire, the moveable wire within rapidly rotates round the magnet thus formed at the moment; and by changing either the connexion, or the pole of the magnet in contact with the iron, the direction of the motion itself is changed.

The small apparatus in the plate is not drawn to any scale. It has been made so small as to produce rapid revolutions, by the action of two plates of zinc and copper, containing not more than a square inch of surface each.

In place of the ball and socket-joint (fig. 3 and 4) loops may be used: or the fixed wire may terminate in a small cup containing mercury, with its aperture upwards, and the moveable wire may be bent into the form of a hook, of which the extremity should be sharpened, and rest in the mercury on the bottom of the cup.

Note on New Electro-Magnetical Motions[1].

At page 147 of this volume, I mentioned the expectation I entertained of making a wire through which a current of voltaic electricity was passing, obey the magnetic poles of the earth in the way it does the poles of a bar magnet. In the latter case it rotates, in the former I expected it would vary in weight; but the attempts I then made, to prove the existence of this action, failed. Since then I have been more successful, and the object of the present note is so far to complete that paper, as to show in what manner the rotative force of the wire round the terrestrial magnetic pole, is exerted, and what the effects produced by it, are.

Considering the magnetic pole as a mere centre of action, the existence and position of which may be determined by well-known means, it was shown by many experiments, in the paper, page 127, that the electro-magnetic wire would rotate round the pole, without any reference to the position of the axis joining it with the opposite pole in the same bar; for sometimes the axis was horizontal, at other times vertical, whilst the rotation continued the same. It was also shown that the wire, when influenced by the pole, moved laterally, its parts describing circles in planes perpendicular nearly to the wire itself. Hence the wire, when straight and confined to one point above, described a cone in its revolution, but when bent into a crank, it described a cylinder; and the effect was evidently in all cases for each point of the wire to describe a circle round the pole, in a plane perpendicular to the current of electricity through the wire. In dispensing with the magnet, used to give these motions, and operating with the terrestrial magnetic pole, it was easy, by applying the information gained above, to deduce before-hand the direction the motions would probably take; for, assuming that the dipping-needle, if it does not point to the pole of the earth, points at least in the direction in which that pole is active, it is evident that a straight electro-magnetic wire, affected by the terrestrial as by an artificial pole, would move laterally at right angles to the needle; that is to say, it would endeavour to describe a cylinder round the pole, the radius of which may be represented by the line of the needle prolonged to the pole itself. As these cylinders, or circles, would be of immense magnitude, it was evident that only a very minute portion of them could be brought within the reach of the experiment; still, however, that portion would be sufficient to indicate their existence, inasmuch as the motions taking place in the part under consideration, must be of the same kind, and in the same direction, as in every other part.

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[1] Quarterly Journal of Science, xii. 416.

Reasoning thus, I presumed that an electro-magnetic wire should move laterally, or in a line perpendicular to the current of electricity passing through it, in a plane perpendicular to the dipping-needle; and the dip being here 72° 30′, that plane would form an angle with the horizon of 17° 30′, measured on the magnetic meridian. This is not so far removed from the horizontal plane, but that I expected to get motions in the latter, and succeeded in the following manner:—A piece of copper wire, about ·045 of an inch thick, and fourteen inches long, had an inch at each extremity bent at right angles, in the same direction, and the ends amalgamated; the wire was then suspended horizontally, by a long silk thread from the ceiling. A basin of clean pure mercury was placed under each extremity of the wire and raised until the ends just dipped into the metal. The mercury in both basins was covered by a stratum of diluted pure nitric acid, which dissolving any film, allowed free motion. Then connecting the mercury in one basin with one pole of Hare's calorimotor, the instrument mentioned page 127, the moment the other pole was connected with the other basin, the suspended wire moved laterally across the basins till it touched the sides: on breaking the connexion, the wire resumed its first position; on restoring it, the motion was again produced. On changing the position of the wire, the effect still took place; and the direction of the motion was always the same relative to the wire, or rather to the current passing through it being at right angles to it. Thus when the wire was east and west, the east end to the zinc, the west end to the copper plate, the motion was towards the north; when the connexions were reversed, the motion was towards the south. When the wire hung north and south, the north end to the zinc plate, the south end to the copper plate, the motion was towards the west; when the connexions were reversed, towards the east; and the intermediate positions had their motions in intermediate directions.

The tendency, therefore, of the wire to revolve in a circle round the pole of the earth, is evident, and the direction of the motion is precisely the same as that pointed out in the former experiments. The experiment also points out the power which causes Ampère's curve to traverse, and the way in which that power is exerted. The well-known experiment, made by M. Ampère, proves, that a wire ring, made to conduct a current of electricity, if it be allowed to turn on a vertical axis, moves into a plane east and west of the magnetic meridian; if on an east and west horizontal axis, it moves into a plane perpendicular to the dipping-needle. Now if the curve be considered as a polygon of an infinite number of sides, and each of these sides be compared in succession to the straight wire just described, it will be seen that the motions given to them by the terrestrial pole, or poles, are such as would necessarily bring the polygon they form into a plane perpendicular to the dipping-needle; so that the traversing of the ring may be reduced to the simple rotation of the wire round a pole. It is true the whole magnetism of the earth is concerned in producing the effect, and not merely that portion which I have, for the moment, supposed to respect the north pole of the earth as its centre of action; but the effect is the same, and produced in the same manner; and the introduction of the influence of the southern hemisphere, only renders the result analogous to the experiment at page 134, where two poles are concerned, instead of that at page 129, &c., where one pole only is active.

Besides the above proof of rotation round the terrestrial pole, I have made an experiment still more striking. As in the experiment of rotation round the pole of a magnet, the pole is perpendicular to but a small portion of the wire, and more or less oblique to the rest, I considered it probable, that a wire, very delicately hung, and connected, might be made to rotate round the dip of the needle by the earth's magnetism alone; the upper part being restrained to a point in the line of the dip, the lower being made to move in a circle surrounding it. This result was obtained in the following manner: a piece of copper wire, about 0·018 of an inch in diameter, and six inches long, was well amalgamated all over, and hung by a loop to another piece of the same wire, as described at page 151, so as to allow very free motion, and its lower end was thrust through a small piece of cork, to make it buoyant on mercury; the upper piece was connected with a thick wire, that went away to one pole of the voltaic apparatus; a glass basin, ten inches in diameter, was filled with pure clear mercury, and a little dilute acid put on its surface as before; the thick wire was then hung over the centre of the glass basin, and depressed so low that the thin moveable wire having its lower end resting on the surface of the mercury, made an angle of about 40° with the horizon. Immediately the circuit through the mercury was completed, this wire began to move and rotate, and continued to describe a cone whilst the connexions were preserved, which though its axis was perpendicular, evidently, from the varying rapidity of its motion, regarded a line parallel to the dipping-needle as that in which the power acted that formed it. The direction of the motion was, as expected, the same as that given by the pole of a magnet pointing to the south. If the centre from which the wire hung was elevated until the inclination of the wire was equal to that of the dip, no motion took place when the wire was parallel to the dip; if the wire was not so much inclined as the dip, the motion in one part of the circle capable of being described by the lower end was reversed; results that necessarily follow from the relation of the dip and the moving wire, and which may easily be extended.

I have described the effects above as produced by the north-pole of the earth, assuming that pole as a centre of action, acting in a line represented by the dip of the needle. This has been done that the phenomena might more readily be compared with those produced by the pole of a magnet. M. Biot has shown by calculation that the magnetic poles of the earth may be considered as two points in the magnetic axis very near to each other in the centre of the globe. M. Ampère has in his theory advanced the opinion that the magnetism of the earth is caused by electric currents moving round its axis parallel to the equator. Of the consonance existing among the calculation, the theory and the facts, some idea may perhaps be gained from what was said, page 138, on the rotation of a pole through and round a wire ring. The different sides of the plane which pass through the ring, there described, and which may represent the equator in M. Ampère's theory, accord perfectly with the hemispheres of the globe; and the relative position of the supposed points of attraction and repulsion, coincide with those assigned by M. Biot for the poles of the earth itself. Whatever, however, may be the state and arrangement of terrestrial magnetism, the experiments I have described bear me out, I think, in presuming, that in every part of the terrestrial globe an electro-magnetic wire, if left to the free action of terrestrial magnetism, will move in a plane (for so the small part we can experiment on may be considered) perpendicular to the dip of the needle, and in a direction perpendicular to the current of electricity passing through it.

Reverting now to the expectation I entertained of altering the apparent weight of a wire, it was founded on the idea that the wire, moving towards the north round the pole, must rise, and moving towards the south, must descend; inasmuch as a plane perpendicular to the dipping-needle, ascends and descends in these directions. In order to ascertain the existence of this effect, I bent a wire twice at right angles, as in the first experiment described in this note, and fastened on to each extremity a short piece of thin wire amalgamated, and made the connexion into the basins of mercury by these thin wires. The wire was then suspended, not as before, from the ceiling, but from a small and delicate lever, which would indicate any apparent alteration in the weight of the wire; the connexions were then made with a voltaic instrument, but I was surprised to find that the wire seemed to become lighter in both directions, though not so much when its motion was towards the south as towards the north. On further trial it was found to ascend on the contacts being made, whatever its position to the magnetic meridian, and I soon ascertained that it did not depend on the earth's magnetism, nor on any local magnetic action of the conductors, or surrounding bodies on the wire.

After some examination I discovered the cause of this unexpected phenomenon. An amalgamated piece of the thin copper wire was dipped into clean mercury, having a stratum of water or dilute acid over it; this, however, was not necessary, but it preserved the mercury clean and the wire cool. In this position the cohesive attraction of the mercury raised a little elevation of the metal round the wire of a certain magnitude, which tended to depress the wire by adding to its weight. When the mercury and the wire were connected with the poles of the voltaic apparatus, this elevation visibly diminished in magnitude by an apparent alteration in the cohesive attraction of the mercury, and a part of the force which before tended to depress the wire was thus removed. This alteration took place equally, whatever the direction in which the current was passing through the wire and the mercury, and the effect ceased the moment the connexions were broken.

Thus the cause which made the wire ascend in the former case was evident, and by knowing it, it was easy to construct an apparatus in which the ascent should be very considerable. A piece of copper bell wire, about two inches long, had portions of the amalgamated fine copper wire soldered on to its ends, and those bent downwards till parallel to each other. This was then hung by a silk thread from the lever, and the fine wire ends dipped into two cups of clean mercury. When the communications were completed from the voltaic instrument through these two cups, the wires would rise nearly an inch out of the mercury, and descend again on breaking the communication.

Thus it appears that, when a fine amalgamated copper wire dips into mercury, and a current of voltaic electricity passes through the combination, a peculiar effect is produced at the place where the wire first touches the mercury, equivalent to a diminution of the cohesive attraction of the mercury. The effect rapidly diminished by increasing the size of the wire, and 20 pair of plates of Dr. Wollaston's construction, and four inches square, would not produce it with the fine wire: on the contrary, two large plates are sufficient. Dr. Hare's calorimotor was the instrument used, and the charge was so weak that it would barely warm two inches of any sized wire. Whether the effect is an actual diminution of the attraction of the particles of the mercury, or depends on some other cause, remains as yet to be determined. But in any case its influence is so powerful, that it must always be estimated in experiments made to determine the force and direction of an electro-magnetic wire, acted on by a magnetic pole, if the direction is otherwise than horizontal, and if they are observed in the way described in this note. Thus, at the magnetic equator, for instance, where the apparent alteration of weight in an electro-magnetic wire may be expected to be greatest, the diminution of weight in its attempt to ascend would be increased by this effect, and the apparently increased gravity produced by its attempt to descend would be diminished, or perhaps entirely counteracted.

I have received an account by letter from Paris, of an ingenious apparatus[1] contrived by M. Ampère, to illustrate the rotatory motions described in my former paper. M. Ampère states that, if made of sufficient size, it will rotate by the magnetic action of the earth, and it is evident that will be the case in latitudes at some distance from the equator, if the rotatory wires, namely, those by which the ring of zinc is suspended, are in such a position as to form an angle with a vertical line, larger than that formed by the direction of the dip.

It is to be remarked that the motions mentioned in this note were produced by a single pair of plates, and therefore, as well as those described in the paper, page 127, are the reverse of what would be produced by two or more pair of plates. It should be remembered also, that the north pole of the earth is opposite in its powers to what I have called the north poles of needles or magnets, and similar to their south poles.

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[1] See Quarterly Journal of Science, xii. 415.

I may be allowed, in conclusion, to express a hope that the law I have ventured to announce, respecting the directions of the rotatory motions of an electro-magnetic wire, influenced by terrestrial magnetism, will be put to the test in different latitudes; or, what is nearly the same thing, that the law laid down by M. Ampère, as regulating the position taken by his curve, namely, that it moves into a plane perpendicular to the dipping-needle, will be experimentally ascertained by all those having the opportunity.

Historical Sketch, &c.

Prior to and just before September 1821, I had been engaged in writing an 'Historical Sketch of Electro-Magnetism,' which may be found published in the Annals of Philosophy, New Series, for September and October 1821, and February 1822, or in volumes ii. 195, 274, and iii. 107. The thoughts which then arose led to the preceding papers and the discovery of *Electro-Magnetic* rotation. As papers further on refer to it for *dates*, I think it needful to indicate here where it may be found, though I do not think it necessary to reprint the account, as it describes the facts of others and not of myself.—Mar. 1844.

Effect of Cold on Magnetic Needles[1].

Dr. De Sanctis has lately published some experiments on the effect of cold in destroying the magnetic power of needles[2], or at least in rendering them insensible to the action of iron and other magnets. Mr. Ellis has claimed the merit of this discovery, and the reasoning upon it, for the late Governor Ellis. Conceiving it important to establish the fact, that cold as well as heat injured or destroyed the magnetic power of iron or steel, we wrapped a magnetic needle up in lint, dipped it in sulphuret of carbon, placed it on its pivot under the receiver of an air-pump, and rapidly exhausted: in this way a cold, below the freezing of mercury, is readily obtained. When in this state, the needle was readily affected by iron or a magnet, and the number of vibrations performed in a given time by the influence of the earth upon it were observed. A fire was now placed near the pump, and the whole warmed; and when at about 80° Fahr. the needle was again examined, it appeared to be just in the same state as before as to obedience to iron and a magnet, and the number of oscillations were very nearly the same, though a little greater. The degree of exhaustion remained uniform throughout the experiment.—Ed.

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[1] Quarterly Journal of Science, xiv. 435.

[2] Phil. Mag. lx. 199.

Historical Statement respecting Electro-Magnetic Rotation[1].

In the xiith volume of the Quarterly Journal of Science, at page 74, I published a paper on some new electro-magnetical motions, and on the theory of magnetism (p. 127.). In consequence of some discussion, which arose immediately on the publication of that paper, and also again within the last two months, I think it right, both in justice to Dr. Wollaston and myself, to make the following statement:—

Dr. Wollaston was, I believe, the person who first entertained the possibility of electro-magnetic rotation; and if I now understand aright, had that opinion very early after repeating Professor Œrsted's experiments. It may have been about August 1820, that Dr. Wollaston first conceived the possibility of making a wire in the voltaic circuit revolve on its own axis. There are circumstances which lead me to believe that I did not hear of this idea till November following; and it was at the beginning of the following year that Dr. Wollaston, provided with an apparatus he had made for the purpose, came to the Institution with Sir Humphry Davy, to make an experiment of this kind. I was not present at the experiment, nor did I see the apparatus, but I came in afterwards, and assisted in making some further experiments on the rolling of wires on edges[2]. I heard Dr. Wollaston's conversation at the time, and his expectation of making a wire revolve on its own axis; and I suggested (hastily and uselessly) as a delicate method of

[1] Quarterly Journal of Science, xv. 288.

[2] See Sir Humphry Davy's Letter to Dr. Wollaston, Phil. Trans. 1821, p. 17

suspension, the hanging the needle from a magnet. I am not able to recollect, nor can I excite the memory of others to the recollection of the time when this took place. I believe it was in the beginning of 1821.

The paper which I first published was written, and the experiments all made, in the beginning of September, 1821. It was published on the first of October; a second paper was published in the same volume on the last day of the same year. I have been asked, why in those papers I made no reference to Dr. Wollaston's opinions and intentions, inasmuch as I always acknowledged the relation between them and my own experiments. To this I answer, that upon obtaining the results described in the first paper, and which I showed very readily to all my friends, I went to Dr. Wollaston's house to communicate them also to him, and to ask permission to refer to his views and experiments. Dr. Wollaston was not in town, nor did he return whilst I remained in town; and, as I did not think I had any right to refer to views not published, and as far as I knew not pursued, my paper was printed and appeared without that reference whilst I remained in the country. I have regretted ever since I did not delay the publication, that I might have shown it first to Dr. Wollaston.

Pursuing the subject, I obtained some other results which seemed to me worthy of being known. Previous to their arrangement in the form in which they appear at page 416 of the same volume (p. 151.), I waited on Dr. Wollaston, who was so kind as to honour me with his presence two or three times, and witness the results. My object was then to ask him permission to refer to his views and experiments in the paper which I should immediately publish, in correction of the error of judgment of not having done so before. The impression that has remained on my mind ever since (one and twenty months) and which I have constantly expressed to every one when talking on the subject, is, that he wished me not to do so. Dr. Wollaston has lately told me that he cannot recollect the words he used at the time; that, as regarded himself, his feelings were it should not be done, as regarded me, that it should; but that he did not tell me so. I can only say that my memory at this time holds most tenaciously the following words: "I would rather you should not;" but I must, of course, have been mistaken. However, that is the only cause why the above statement was not made in December 1821; and that cause being removed, I am glad to make it at this, the first opportunity.

It has been said I took my views from Dr. Wollaston. That I deny; and refer to the following statement, as offering some *proof* on that point. It has, also, been said, that I could never, unprepared, have gained in the course of eight or ten days, the facts described in my first paper. The following information may elucidate that point also.

It cannot but be well known, (for Sir Humphry Davy himself has done me the honour to mention it) that I assisted him in the important series of experiments he made on this subject. What is more important to me in the present case, however, is not known; namely, that I am the author of the *Historical Sketch of Electro-magnetism,* which appeared in the Annals of Philosophy, New Series, vols. ii. and iii. Nearly the whole of that sketch was written in the months of July, August, and September of 1821; and the first parts, to which I shall particularly refer, were published in September and October of the same year. Although very imperfect, I endeavoured, as I think appears on the face of the papers, as far as in me lay, to make them give an accurate account of the state of that branch of science. I referred, with great labour and fatigue, to the different journals in which papers by various philosophers had appeared, and repeated almost all the experiments described.

Now this sketch was written and published *after* I had heard of Dr. Wollaston's expectations, and assisted at the experiments before referred to; and I may, therefore, refer to it as a public testimony of the state of my knowledge on the subject *before* I began my own experiments. I think any one, who reads it attentively, will find, in every page of the first part of it, proofs of my ignorance of Dr. Wollaston's views; but I will refer more particularly to the paragraph which connects the 198th and 199th pages, and especially to the 18th and 19th lines of it; and also to fig. 4 of the accompanying plate. There is there an effect described in the most earnest and decided manner (see the next paragraph but one to that referred to); my accuracy, and even my ability, is pledged upon it; and yet Dr. Wollaston's views and reasonings, which it is said I knew, are founded, and were, from the first, as I now understand, upon the knowledge of an effect quite the reverse of that I have stated. I describe a neutral position when the needle is opposite to the wire; Dr. Wollaston had observed, from the first, that there was no such thing as a neutral position, but that the needle passed by the wire: I, throughout the sketch, describe attractive and repulsive powers on each side of the wire; but what I thought to be attraction to, and repulsion from the wire in August 1821, Dr. Wollaston long before perceived to arise from a power not directed to or from the wire, but acting circumferentially round it as axis, and upon that knowledge founded his expectation.

I have before said, I repeated most of the experiments described in the papers referred to in the sketch; and it was in consequence of repeating and examining this particular experiment, that I was led into the investigation given in my first paper. He who will read that part of the sketch, above referred to[1], and then the first, second and third pages of my paper[2], will, I think, at once see the connexion between them; and from my difference of expression in the two, with regard to the attractive and repulsive powers, which I at first supposed to exist, will be able to judge of the new information which I had, at the period of writing the latter paper, then, for the first time acquired.

Electro-magnetic Current (*under the Influence of a Magnet*[3]).

As the current of electricity, produced by a voltaic battery when passing through a metallic conductor, powerfully affects a magnet, tending to make its poles pass round the wire, and in this way moving considerable masses of matter, it was supposed that a reaction would be exerted upon the electric current capable of producing some visible effect; and the expectation being, for various reasons, that the approximation of a pole of a powerful magnet would diminish the current of electricity, the following experiment was made. The poles of a battery of from two to thirty 4-inch plates were connected by a metallic wire formed in one part into a helix with numerous convolutions, whilst into the circuit, at another part, was introduced a delicate galvanometer. The magnet was then put, in various positions, and to different extents, into the helix, and the needle of the galvanometer noticed; no effect, however, upon it could be observed. The circuit was made very long, short, of wires of different metals and different diameters down to extreme fineness, but the results were always the same. Magnets more and less powerful were used, some so strong as to bend the wire in its endeavours to pass round it. Hence it appears, that however powerful the action of an electric current may be upon a magnet, the latter has no tendency, by reaction, to diminish or increase the intensity of the former;—a fact which, though of a negative kind, appears to me to be of some importance.—M. F. [See note at end of Series 1. of Exp. Res. 1843.]

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[1] Annals of Philosophy, N. S., ii. 198, 199.

[2] Quarterly Journal, xii. 74–76, or pp. 127–129 of this volume.

[3] Quarterly Journal of Science, xix. 338.

Electric Powers (and place) of Oxalate of Lime[1].

Some oxalate of lime, obtained by precipitation, when well-washed, was dried in a Wedgewood's basin at a temperature approaching 300°, until so dry as not to render a cold glass plate, placed over it, dim. Being then stirred with a platina spatula, it, in a few moments, by friction against the metal, became so strongly electrical, that it could not be collected together, but flew about the dish whenever it was moved, and over its sides into the sand-bath. It required some little stirring before the particles of the powder were all of them sufficiently electrical to produce this effect. It was found to take place either in porcelain, glass, or metal basins, and with porcelain, glass, or metal stirrers; and when well excited, the electrified particles were attracted on the approach of all bodies, and when shaken in small quantity on to the cap of a gold-leaf electrometer, would make the leaves diverge two or three inches. The effect was not due to temperature, for when cooled out of the contact of air, it equally took place when stirred; being, however, very hygrometric, the effect soon went off if the powder were exposed to air. Excited in a silver capsule, and then left out of contact of the air, the substance remained electrical a great length of time, proving its very bad conducting power; and in this respect surpassing, perhaps, all other bodies. The effect may be produced any number of times, and after any number of desiccations of the salt.

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[1] Quarterly Journal of Science, xix. 338.

Platina rubbed against the powder became negative—the powder positive; all other metals tried, the same as platina. When rubbed with glass, the glass became strongly negative, the oxalate positive, both being dry and warm; and indeed this body appears to stand at the head of the list of all substances as yet tried, as to its power of becoming positively electrical by friction.

Oxalates of zinc and lead produced none of these effects.—M. F.

On the Electro-motive Force of Magnetism. By Signori Nobili *and* Antinori *(from the* Antologia, No. 131)*: with Notes by* Michael Faraday, *F.R.S., &c*[1].

Mr. Faraday has recently discovered a new class of electrodynamic phenomena. He has presented a memoir on this subject to the Royal Society of London, which is not yet published, and of which we have received the simple notice, communicated by M. Hachette to the Academy of Sciences at Paris on the 26th of December last, in consequence of a letter which he had received from Mr. Faraday himself[2]. This relation in-

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[1] Philosophical Magazine and Annals, 1832, xi. 402.

In this paper the date on the right-hand page is that of my notes, that on the left-hand is meant to be the one of Signori Nobili and Antinori's paper. Of the latter however there is great doubt, for the date attached by the writer is 31st January, 1832, whilst the number of the Antologia in which it appears professes to have for date, November 1831. The latter is probably the false date, and so the real date of publication is unknown; it could not however be before February 1832.

[[2] I am glad of an opportunity of adding a few notes to a public version of Sig. Nobili and Antinori's paper. My hasty letter to M. Hachette, in consequence, probably, of my bad writing, has been translated with some errors; and has been, by Sig. Nobili at least, seriously misunderstood. Had it remained private, it would not have been of much consequence: but as it duced Cav. Antinori and myself immediately to repeat the fundamental experiment, and to study it under its various aspects. As we flatter ourselves we have arrived at results of some importance, we hasten to publish them without any other preamble than the same notice which has served as the point of departure in our researches.

"The memoir of Mr. Faraday," so says the notice, "is divided into four parts. In the first, intitled 'Production of Voltaic Electricity[1],' is found the following important fact,—that a voltaic current which traverses a metallic wire produces another current in a neighbouring wire; that the second current is in a direction contrary to the first, and continues but for a moment; that if the producing current is removed, a second current is manifested in the wire submitted to its action contrary to that which was first formed in it, *i. e.* in the same direction as the producing current.

"The second part of the memoir treats of electric currents produced by the magnet. On causing helices to approach to magnets, Mr. Faraday has produced electric currents; on removing the spirals, currents in the contrary direction were formed. These currents act powerfully on the galvanometer; pass, though feebly, through brine and other solutions, and in a particular case Mr. Faraday *has obtained a spark*. Hence it follows that this philosopher has by using a magnet only produced the electric currents discovered [studied] by M. Ampère.

"The third part of the memoir is relative to a particular

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has appeared in three or four languages, and forms the text of all subsequent papers on magnetic electricity, it is very requisite to correct certain errors which have arisen from it, especially that of Sig. Nobili relative to Arago's rotation.

My first paper was read to the Royal Society, November 24, 1831; and my letter to M. Hachette was dated the 17th of December, 1831; my second paper was read January 12th, 1832. Sig. Nobili's paper is dated January 31st, 1832. Signori Nobili and Antinori worked only from my letter to M. Hachette; but as I hope I may claim whatever is contained in my two papers, I have introduced into the present paper references, in figures included within parentheses, to paragraphs in my papers, wherever the experiments described are either altogether, or only to a partial extent, repetitions of my results.—M. F.]

[[1] This should be *induction of voltaic electricity*.—M. F.]

electric state, which Mr. Faraday calls *electrotomo state*[1]. He intends to write of this another time.

"The fourth part speaks of the experiment not less curious than extraordinary of M. Arago, which consists, as is known, in making a magnetic needle revolve under the influence of a rotatory metallic disc, and *vice versâ*. Mr. Faraday considers this phenomenon as intimately connected with that of the magnetic rotation, which he had the fortune to discover about ten years ago. He has ascertained that by the rotation of the metallic disc under the influence of a magnet, there may be formed electric currents in the direction of the rays of the disc in sufficient number to render the disc a new electrical machine." —*Le Temps*, Dec. 28, 1831.

1. *Ordinary Magnetism* (Phil. Trans. 1832. Part I. *Experimental Researches in Electricity*, 27 *to* 59: 83 *to* 138 : 217 *to* 264).

We had no occasion to make trials before we succeeded in the experiment of Mr. Faraday. The first spirals which we brought near to the pole of a magnet quickly manifested their influence on the galvanometer. We observed three facts in succession (*Exp. Res.* 30. 37. 47.). Whilst approaching the magnet, the needle of the instrument is in the first place seen to deviate a certain number of degrees, which indicates a current excited by the magnetism, in the spirals previously made to communicate with the galvanometer. This current lasts but for a moment, and is then completely extinct, as is proved by the needle returning to its first position: this is the second observation. The third (finally) occurs when the spiral is taken from the magnet: the needle of the galvanometer then deviates on the other side, demonstrating the development of a current contrary to that excited in the first instance.

On experimenting with an annular spiral between the poles of a horse-shoe magnet, we observed that the action was much less than that produced with the same spiral when the lifter of the magnet was put to it or suddenly taken from it (*Exp. Res.* 34.). This fact suggested the idea of rolling a copper wire covered with silk round such a magnet, so as to have an apparatus always mounted for the experiment in question. The spiral to be subjected to the magnetic influence is then always upon the magnet, and the immediate cause of the phenomena resides in the lifter, because of the property which that little piece of soft iron possesses of being magnetized and de-magnetized rapidly. When the lifter is detached, the spiral which before was in the presence of this piece of iron strongly magnetized, is suddenly removed from its action, and represents the case of a spiral which having been first approximated to a magnet is then removed. When the lifter is replaced, it is as if a magnet were caused to approach the spiral, for the lifter becomes magnetic on being attached to the poles of its own magnet.

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[1] This should be *electrotonic state*. I said I should write to my friend about it another time.—M. F.]

This arrangement, besides being very active, has the advantage of supplying the philosopher with a *constant source* of voltaic electricity (*Exp. Res.* 46 note). The want of a constant current is often felt in such researches; and if thermo-magnetism offers a plausible means of satisfying such necessities, as I have indicated elsewhere [1], yet the new method offered us by a magnet covered with electro-dynamic spirals is not to be despised. Here the currents are always ready to be manifested. Suppose, as is usual, the lifter of the magnet is in its place, nothing more is required to obtain a current in the spiral than to detach the lifter, the current in the wire being, as it were, at first in a latent state.

There are two modes of using this arrangement; the one by attaching the lifter, the other by detaching it. When the two motions are made with the same rapidity, and with relation to the same points of the magnet, the deviations are in the inverse directions to each other, but precisely of the same value. The detachments are, however, always equally instantaneous, and for constancy of effect are preferable to approximations; for the latter to be always equally successful would require a mechanical arrangement, which it is not worth while either to imagine or to execute. By taking care that the lifter is constantly in its right place and position, there will always be produced the same deviation of the galvanometer when it is detached from the magnet. This we repeat is a valuable result applicable in numerous cases, and perhaps proper to measure the force of large magnets in a more exact manner than by the ordinary mode of ascertaining the weights sustained.

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[1] This means consists in having a thermo-electric elementary combination composed of two metals only, and heated at one juncture to 0° Fahr., at the other to 212° Fahr.—*Ann. de Chimie*, Feb. 1830, p. 130.

The arrangement described is highly advantageous; but does it produce the maximum of electro-dynamic effects? There is indeed another much better (*Exp. Res.* 46 note), which consists in applying the electro-dynamic spiral to the central part of the lifter, corresponding to the interval which separates the poles of the horse-shoe magnet. In this position a spiral of a few turns is able to surpass the effects of a far greater number of spirals otherwise disposed. Behold then the arrangement which it is convenient to make to obtain all the effects of a magnet. The central part of the lifter is to be entirely covered with wire, leaving exposed only the extremities, which are to come in contact with the pole of the magnet. The ordinary form of the lifter is not the most convenient upon which to arrange this species of large electro-dynamic ring, but upon conveniently modifying its shape the wire may be applied with facility, and thus the effect be obtained at its highest degree of intensity. The reason is evident; for two conditions in fact require to be fulfilled: one, that the spiral should be subjected to all the influence of the magnetic force; the other, that this influence should be abstracted in the shortest possible time. Now the wire round the lifter is exactly in the most favourable position for the magnetic force to be concentrated upon it; and this force vanishes the instant the lifter is detached, as is required by the second condition.

Spirals of various Metals (*Exp. Res.* 132. 139. 193. 208. &c.).

The metals with which we have experimented are four,—copper, iron, bismuth, and antimony: iron is interesting as the foremost amongst magnetic metals (*Exp. Res.* 8. 9. 211.); bismuth and antimony for the distinct position they hold in the thermo-magnetic scale. In experiments made under circumstances approximating to equality, it appeared that copper was the most active in the present point of view; then at a little distance iron (*Exp. Res.* 207. 212.); afterwards antimony; and finally, bismuth. But in truth the fragility of the two latter only allowed us to give them the spiral figure by fusing them. For this method, which was long and difficult, we supplied another; which was, to make quadrangular spirals of a number of rods of these metals soldered at their extremities, or else merely held and pressed the one against the other, to ensure contact. It is scarcely necessary to say, that in order to obtain comparative results the same quadrangular form was given to the spirals of copper and iron.

2. *Electric Spark* (*Exp. Res.* 32. 57.[1]).

The relation placed at the head of this article says, "*that in a particular case Mr. Faraday had obtained a spark*" (*Exp. Res.* 32.). Although this expression gave no light on the subject, and rather rendered doubtful the constancy of so extra-

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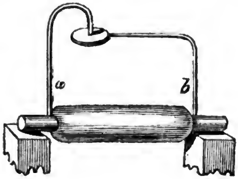
[[1] Being much engaged in the investigation and confirmation of the laws of magneto-electric action, terrestrial magnetic induction, &c. &c. some of the results of which are contained in my second paper (The Bakerian Lecture), it will be seen that in the race which Sig. Nobili and Antinori (probably inadvertently) ran against me (see the last paragraph of their paper), they obtained the electric spark from the common magnet before me. I have great pleasure in bearing witness to the accuracy of their reasoning on this point, and also to the success of the result. Having made a variation of the experiment by obtaining the spark from the action of a common loadstone, in which their most perfect mode could not be applied, I will take the opportunity of describing the simple adjustment I have devised. A helix was fixed round the lifter, the wire ends were raised upwards; one, which may be called *a*, was bent into a hook as in the figure; the other, *b*, after rising was bent at a right angle, and had a thick small circular plate of copper fixed to it, which was made by the spring of the wire to press in the middle slightly against the rounded end of *a*; this plate and the end of *a* were amalgamated. On bringing the lifter down suddenly upon the poles in the position figured, the momentum of the plate caused it to separate from the end of *a*, and the spark passed. On lifting it up the concussion always separates the end of *a* from the plate, and a spark is again seen. When the plate and the point are well amalgamated, the spark will not fail once in a hundred times either at making or breaking contact. I have shown it brilliantly to two or three hundred persons at once, and over all parts of the theatre of the Royal Institution.



As Professor Ritchie expresses it, the spark has not yet been obtained except from a temporary magnet, *i. e.* from a magnet in the act of being made or ordinary a phenomenon, we nevertheless did not suspend our researches, and have been so fortunate as to succeed beyond our hopes. The following are the theoretical views which have conducted us to this important result, but which, we fairly say, at first gave us but very little confidence.

The voltaic pile gives a spark only when composed of a certain number of pairs of plates. A single Wollaston's voltaic element yields it; and when of a certain activity produces it constantly at the surface of mercury, to which the conjoining wires destined to close the circuit are conducted. In the voltaic pile having a certain degree of *electric tension*, the sparks pass between the zinc and copper poles, either in the case of opening or of closing the circuit. In a single Wollaston's element the tension is feeble, and the spark occurs only when the circuit is interrupted. At that moment the current which before was moving, accumulates as it were at the place of interruption, and acquires the intensity necessary to cause the spark. Such tension is wanting in the other case of closing the circuit, and the spark also is absent.

The currents developed in the electro-dynamic spirals by virtue of magnetism are also in motion, but circulate only for the moment during which they are approaching to or receding from the magnet. It was therefore, we concluded, in one of those two moments that we ought to open the circuit in making the experiment for the spark.

Thus we arranged our ideas relative to the best disposition of the electro-dynamic spirals; nothing therefore remained but to select a good horse-shoe magnet; to surround the lifter

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destroyed. I obtained the first spark from a soft iron magnet made by the well-known influence of electric currents. Sig. Nobili and Antinori obtained the second spark from a soft iron magnet made so by the influence of a common artificial steel magnet; their result has been repeated by a great number of persons. Mr. Forbes of Edinburgh first obtained the spark from a soft iron magnet made so by the influence of the natural loadstone. The latter experiment is also that which I have made with Mr. Daniell's loadstone, lifting only about thirty pounds, and in the manner described. I was not aware of any other modes of performing the experiment except my original one, and Sig. Nobili and Antinori's.—M. F.] Since this time I have obtained the spark a step nearer to the inducting magnet than in any of these cases: see onwards at date of November 1834, or Phil. Mag. 1834, v. p. 350.—December 1843.

with a copper wire in the manner before described; to immerse the extremities of this wire in a cup of mercury, and to raise the one or the other extremity at that precise moment when the lifter was attached to or detached from the magnet. When two persons operate without any kind of machinery, it is more easy to lose than to catch this moment. But when the movements were simultaneous, which happened every now and then, we had the satisfaction of seeing a spark, which left nothing to be desired.

Such was the mode by which we saw the first spark: but as this beautiful result deserved to be produced at pleasure, it claimed an appropriate apparatus; and after various arrangements more or less complicated, we stopped at the following, which has the advantage of being very successful and very simple.

The whole of the contrivance is attached to the lifter of the magnet. This piece, which is a parallelopiped, is surrounded in the middle by the electro-dynamic spiral, to which it is firmly attached by two pieces of brass, so that the latter can enter between the magnetic poles whilst the lifter comes in contact with the poles in the ordinary way. The extremities of the spiral come in contact one with each magnetic pole by means of two little springs in the form of wings attached to the lifter, and which press slightly against the poles when the lifter is in its place. To leave room for these springs, the lifter is narrower than usual, covering about half the poles; the remaining space serves for the contact of the springs, which are in this way isolated as it were from the lifter; and yet by means of the magnet itself serve to complete the electro-dynamic circuit. Suppose that the lifter is in its place, the springs touch the poles, and the circuit of the spirals is metallically closed by the magnets; on detaching the lifter, the circuit opens in two places; and either at the one or the other interruption the spark almost constantly appears. When the effect does not take place, it is because the separation has not been well effected; but it is so easy to repeat the experiment, that it is useless to think of a piece of mechanism to remedy an inconvenience which is so easily remedied.

In this apparatus the spiral on the lifter was of copper. On substituting an iron wire the spark also occurred. This experiment was interesting in illustration of any influence which the ordinary power of the magnet over iron might exert upon the electro-dynamic influence. It did not appear that the one action disturbed the other; but before positively affirming the independence, it will be necessary to obtain other proof, which we shall endeavour to do at a more favourable opportunity (*Exp. Res.* 9. 254.).

3. *Terrestrial Magnetism* (*Exp. Res.* 137. 140. &c.).

We took a paper tube two inches in diameter and four inches long, a copper wire forty metres long was coiled round it, the two ends being left at liberty to connect with the galvanometer; the tube was trimmed at the ends, so that it could be placed upright upon the table either in one direction or the other at pleasure (*Exp. Res.* 142.). A cylinder of soft iron, as is well known, placed parallel to the dip is subject to the terrestrial magnetic influence; the lower part becomes a north pole, the upper a south pole. This is a phenomenon of position always occurring in the same direction with this kind of iron, which is as incapable of retaining the magnetism received, as it is disposed to receive the new magnetism to which it may be subjected.

In our latitudes the inclination of the needle is about 63°. The paper tube with its spiral was therefore arranged in that direction, and an iron cylinder introduced; whilst in the act of introducing it, the galvanometer was seen to move (*Exp. Res.* 146.), owing to the presence of an electric current excited by the magnetism. On taking out the cylinder the motion was reversed: there is no doubt, therefore, that terrestrial magnetism is sufficient of itself to develope currents of electricity. It should not be concealed here, that in the above experiment the electricity is developed by the intermedium of soft iron introduced into the spiral: this without doubt is true, but it is also true that it is not essentially necessary to recur to this aid to obtain unequivocal signs of the influence of which we speak. On placing our cylindrical spiral so that its axis should be parallel to the magnetic dip, and then inverting it by a half revolution in the magnetic meridian (*Exp. Res.* 148.), we observed at the comparative galvanometer the signs of a current excited in the spiral by the sole influence of terrestrial magnetism.

It is not even necessary for this effect to place the spiral in the direction of the dip: the experiment will succeed in the vertical position; the effect is less, but always so distinct as to remove every error (*Exp. Res.* 153, &c.).

We experimented with three copper wires of different diameters; the smallest was 0·5, the second 0·66, and the third 1· millimetre in diameter. The effects increased with the size:—the first gave deviations from 2 to 4; the second from 4 to 8; and the third from 10 to 20. To obtain these great motions, we operated in the usual way of inverting the current at the most favourable moment, which is easily learned by repeating the experiment a few times.

In the present state of science this is most certainly the simplest mode of obtaining the current[1]; all is done by terrestrial magnetism, which is everywhere. We purpose hereafter to study the manner of increasing the effect, and of making some useful applications, if certain apparatus which we purpose constructing should meet our wishes (*Exp. Res.* 147. 154, &c.). The first thought is that of using it to measure the terrestrial magnetic intensity; but what precision the mode may be capable of, remains at present to be determined.

The galvanometer which should be used for the experiments of this section should be very sensible. And I repeat on this occasion what I have elsewhere said relative to these instruments: two systems may be adopted to obtain maximum effects; the one for hydro-electric currents, the other for thermo-electric currents. The galvanometer of my thermo-multiplicator is of the latter kind, and precisely that which is best in the present researches[2]. The reason will be evident, by observing that the new currents of Faraday are entirely developed in metallic circuits, like the thermo-electricity of Dr. Seebeck; and that, also like those of thermo-electricity, they pass with difficulty through humid conductors.

[[1] A much more simple mode is described in my paper at (170, &c.); for neither spiral nor soft iron is necessary.—M. F.]

[2] Nobili, Bib. Univ., Juillet 1830, p. 275.

4. *Electric Tension.*

The trials which we have as yet made on this new class of currents, to obtain by the electrometer the ordinary signs of tension, have not conducted us to any positive result: but the means which we have employed are far from satisfying us fully. We are preparing others for the purpose of attacking the question with more efficacious means. We shall then extend the research to thermo-electric combinations, which deserve to be studied in the same point of view, as they have never yet presented sensible signs of electric tension. We shall also try with these latter currents to obtain the spark under favourable circumstances; but we cannot but confess that at present we doubt, and consider the thermo-electric currents as in their nature the least fitted to produce either tension or a spark, as we will explain in due time and place.

5. *Chemical and Physiological Effects* (*Exp. Res.* 22. 56. 133.).

The new currents of Faraday pass, although with difficulty, through humid conductors. So says the notice; and such is the fact, as may be readily verified by introducing a conductor of that kind into the circuit of the electro-dynamic spiral (*Exp. Res.* 20. 23. 33. 56.). In the case of other known currents, I have demonstrated elsewhere that there is always chemical decomposition when they pass liquid conductors; and that however feeble they may be, the decomposition is always assured by their transit through the fluid. It is therefore very probable that the new currents will produce the phenomena of decomposition, but their distinctive character of brief duration must not be forgotten (*Exp. Res.* 59, &c.). I believe that the time, however short, is still sufficient for decomposition; but I will not venture anything before I have interrogated that grand master in everything—experiment.

The physiological effects (*Exp. Res.* 22. 56, &c.) consist, as is well known, in the shocks or contractions of the muscles, the acrid and acidulous taste on the tongue, and the light before the eyes[1]. For obtaining these effects, it is absolutely necessary that the electricity should penetrate into our organs;

[[1] The sensation on the tongue and the light before the eyes I believe I have obtained. See (56) of my papers.—M. F.]

these latter belonging to humid conductors. This path, as we have seen, is very difficult for the new currents; nevertheless, the frog put into the circuit of our electro-dynamic spirals, arranged around the lifter of our magnet, was powerfully convulsed each time that the lifter was separated or attached (*Exp. Res.* 56.). The experiment is beautiful and instructive; beautiful, because of the energetic convulsions produced apparently by the immediate action of the magnet; and instructive, because it confirms the fact of the passage of these currents through humid conductors, and because also it shows that the frog is in all cases the most delicate galvanoscope[1]. This is a fit occasion to say what I have already said elsewhere, relative to the discovery of Dr. Seebeck, that it was not necessary that Œrsted's discovery and the following one of the galvanometer should be known, to arrive at the knowledge of the thermo-electric currents[2]. The frog properly prepared was sufficient for the purpose, and the same animal would have been quite sufficient to discover the new currents of Faraday. Although it is not by this road that these two discoveries have been arrived at, still it is not less true that they might have been made by the simple assistance of this interpreter, which astonished Europe in the first times of galvanism.

6. *Magnetism of Rotation* (*Exp. Res.* 81 *to* 139: 149 *to* 169: 181 *to* 192: 217 *to* 230: 244 *to* 254, &c.).

What will happen when an electro-dynamic spiral is approached to the pole of a bar magnet? A current is produced in its successive spirals, which enters upon itself in consequence of the conjunction of the extremities of the wire. But if in place of the spiral a mass of copper is submitted to the influence of the same magnetic pole, what will happen? It would appear reasonable to admit in this mass the same development of currents, with this difference only; that in the spiral they cannot re-enter upon themselves in each spire; whilst in the mass the currents will re-enter directly into themselves, on the circle or zone of matter in which they are determined by the influence of the magnet: these currents, in the present state of science, cannot be considered as other than the consequence of a movement of the same nature which takes place around each particle of the magnetic metal. This induction seems sufficiently natural; and for its greater confirmation we have instituted the following experiment:—a ring of copper was taken, and the two conjoining wires intended to complete the communication with the galvanometer soldered to it at the extremities of one of its diameters. On placing this ring between the two poles of a horse-shoe magnet, in the place where we introduced our electro-dynamic spiral, motions were instantly manifested at the galvanometer, due to the presence of currents excited by the magnetism in the copper ring[1].

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[1] Bib. Univ. xxxvii. 10.

[2] Ibid.

Our idea being thus fixed relative to the circular currents, which we believed ought to be produced in the mass of copper submitted to the influence of the magnetic pole, let us pass to the question of magnetism by rotation, the wonderful discovery of M. Arago. Here we have magnetic poles in presence of a disc, which instead of being quiescent as in the preceding case, is continually moving on its own axis. The latter condition is the only one added, and by it we see that the final result of the phenomena will be excessively complicated, but that in reality nothing new will happen. In all cases it is the currents developed by the magnetism at the place of the disc which is directly acted upon by this magnetism which are concerned. This part is rapidly removed by the rotation, and another comes forward, which is subjected to the same influence, which always tends to form currents in the contrary direction to those which may be supposed to exist in the magnetic pole (*Exp. Res.* 53. 255.). These currents, by their nature, tend to be inverted so soon as they are withdrawn from the presence of the cause which produced them, and are in fact inverted every time that the velocity of rotation will permit it. The theory of this species of magnetism appears mature[2]; we shall endeavour to develope its physical principles in a more detailed manner in a separate paper, being content here

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[[1] This experiment will bear another interpretation. I do not (as I understand the description) believe the ring to have anything particular to do with the result; the whole appears to me a repetition of the experiment I have described (*Exp. Res.* 109).—M. F.]

[[2] Sig. Nobili and Antinori have mistaken the character of the acting causes in Arago's experiment altogether; the view which they have briefly expressed and mean to pursue, is precisely that which I at first entertained and pursued, to state the particular character which distinguishes it from all other kinds, and which rendered it not easily assailable before the discovery of Mr. Faraday. This character does not consist only in momentary duration, which it has in common with soft iron, but also in being a double magnetism, *inverse* and *direct*; *inverse,* at the moment of its production, opposite to the producing cause; *direct,* at the moment after, when this cause disappears.

Mr. Faraday considers Arago's magnetism of rotation as entirely connected with a phenomenon which he discovered about ten years ago (*Exp. Res.* 121.). "*He then ascertained,*" so says the notice, "*that by the rotation of a metallic disc under the influence of a magnet, there may be formed, in the direction of the radii of that disc, electric currents in sufficient number to render the disc a new electric machine.*" We are quite ignorant how Mr. Faraday has ascertained this fact; and we do not know how a result of such a nature could remain so long a time generally unknown, and as it were lost in the hands of the author of the discovery[1]. Besides, there is something here very problematical to us; and before we leave the subject we will describe the experiment we have made relative to it.

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but which I soon found experimental reason to reject. However, I need merely refer here to the fourth division of my first paper, expressly on that phenomenon, and to parts of the sixth division in the continuation of the Researches, for what I believe to be a true view of the phenomenon (see especially *Exp. Res.* 121. 122. 123.).—M. F.]

[[1] Sig. Nobili and Antinori here seriously mistake the sense of my letter to M. Hachette. I did not write "I then ascertained." The French translation of my letter in *Le Lycée*, No. 35, sent to me by M. Hachette, does not say so. "M. Faraday considère le phénomène qui se manifeste dans cette expérience, comme intimement lié à celui de la rotation magnetique qu'il a eu le bonheur de trouver il y a dix ans. Il a reconnu que par la rotation du disc métallique, &c. &c." I am not Italian scholar enough to say how Sig. Nobili and Antinori themselves at first expressed it; but the phrase used in the present part of their paper is, "*Egli reconobbe fin d'allora che, &c.*;" whilst that which they used at the head of the paper, to express the same words of my letter, is, "*Egli ha riconosciuto che, &c. &c.*" It was in consequence of the recent researches detailed in my paper that I ascertained the state of the revolving plate, and could then refer the effect in its kind to that which I had so long before discovered. The succeeding remarks of Sig. Nobili and Antinori have no reference therefore except to their mistake of my meaning.—M. F.]

A disc of copper was revolved, and two long copper wires prepared, attached at one set of ends to the galvanometer, and at the other held by the hand against the disc, the one at the centre, and the other at the circumference, in the direction of the radii. In the rotation of the disc, the points of copper pressed against it will be heated, but unequally; that pressed against the circumference will be most heated, and that at the centre the least. This difference is quite sufficient to determine an electric current capable of moving the needle of the galvanometer, and retaining it after a few vibrations at a certain degree of the division[1]. When the needle is thus quiescent, if a horse-shoe magnet be advanced towards the plate so as to embrace it without interrupting its motion, it will be seen that the deviation of the needle will augment or diminish according as the poles act in the one direction or the other. This effect is a sure proof of the current manifested in the disc by the action of the magnet: but because the wires connected with the galvanometer are arranged with their ends in the direction of the radius of the disc, are we to conclude that they are exactly in the direction in which the current excited by the magnetism exists[2]? We do not believe it, for the reasons given above; and though we should, with Mr. Faraday, admit this species of irradiating currents, there would still exist for us a great difference between this mode of exciting electricity, and the ordinary one of our common electrical machines. There is here a great void to fill, in passing from a superlative conductor, like the metallic disc of M. Arago, to the worst, such as the glass plate of an ordinary machine[3].

[[1] All these causes of error were fully guarded against in every part of my researches (*Exp. Res.* 91. 113. 186.).—M. F.]

[[2] I have nowhere drawn such conclusions.—M. F.]

[[3] The case of the currents tending to be formed, or really existing in the direction of the radii throughout the *whole* plate, occurs only when the axis of the magnet approached coincides with the axis of the revolving plate (*Exp. Res.* 156. 158.), or when the magnetic curves intersected by the revolving plate are of equal strength, and pass through all parts of the plate in the same direction, as happens when the earth's magnetism is used as the exciting cause (*Exp. Res.* 149. 155.). My reasons for calling the revolving plate an electrical machine (*Exp. Res.* 154. 158.) are entirely untouched by what is said in the text.

It must not be supposed that in these notes I am criticizing Sig. Nobili and

But these our particular opinions do not in any way diminish the intrinsic merit of Mr. Faraday's discovery. It is one of the most beautiful of our time, whether it be considered in itself for the largeness of the vacancy which it serves to fill, or for the light which it throws over the various theories, and especially that of magnetism of rotation.

We hope that these our first researches will justify the lively interest which we have taken in this new branch of electro-dynamics. We have but one regret, namely, that of having entered into a path before we knew all the steps taken in it by the illustrious philosopher who threw it open.

Florence, Jan. 31, 1832.

Nobili and Antinori's Errors in Magneto-electric Induction: in a Letter to M. Gay-Lussac[1].

My dear Sir, Royal Institution, Dec. 1, 1832.

I am anxious to write you a letter on Electro-magnetism, and I beg you to insert it in the Annales de Chimie et de Physique, if you can grant me that favour. I fear that this letter may occasion more controversy than I desire, but the circumstances are such as to force me to take the pen; for if I am silent the silence will be regarded as the acknowledgement of error, not only in a philosophical, but in a moral point of view, and that in cases where I believe I am exempt from both.

You doubtless comprehend that I wish to speak of the mémoire of MM. Nobili and Antinori. I write to you because you

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Antinori for not understanding my views. It was impossible that I could put forth in a brief letter, matter which, though I have condensed it as much as possible, still occupies seventy quarto pages of the Philosophical Transactions; and I may perhaps be allowed to say, (more in reference however to what I think ought to be a general regulation than to the present case,) that had I thought that that letter to M. Hachette would be considered as giving the subject to the philosophical world for general pursuit, I should not have written it; or at least not until after the publication of my *first paper*.—M. F.]

[1] Annales de Chimie et de Physique, 1832, t. li. p. 404.

have thought so well of the matter of my paper as to introduce it into your excellent and truly philosophical journal, and because having inserted also the mémoire of MM. Nobili and Antinori, all that has been written on this subject is found in the Annales. I may therefore hope you will not refuse me that which I now desire.

On the 24th November, 1831, my first paper was read to the Royal Society; it is that which you have done me the honour to insert in the Annales for May 1832 (t. l. pp. 5–69). This paper was the *first* announcement which I made of my researches on electricity. The 18th December, 1831, I wrote a letter to my friend M. Hachette, who did me the honour to communicate it to the Academy of Sciences the 26th of the same month[1]. This letter was also inserted in the Number of the Annales for December 1831 (t. xlviii. p. 402). The second series of my researches, dated the 21st December, 1831, was read to the Royal Society the 12th January, 1832, and found a place in the Annales for June 1832 (t. l. pp. 113–162). These are the only publications (except certain notes appended to the memoirs of others) which I have made respecting the present matter up to this time, and the *whole* of them were written and read before anything whatsoever by any other philosopher on the same subject.

In the meantime the letter which I wrote to M. Hachette, and which you did me the honour to insert in the Annales, drew the attention of MM. Nobili and Antinori, and those industrious philosophers published a mémoire, the date of which is 31st January, 1832, and consequently posterior to all my writings. This mémoire obtained a place in the Annales for December 1831 (t. xlviii. pp. 412–430). A second mémoire, entitled "*New Electro-magnetic Experiments*," by the same philosophers, dated the 24th March, 1832, has also appeared, and has been inserted in the Annales for July (t. l. pp. 280–304).

I fear that the letter which I wrote to M. Hachette, and which in his kindness for me he did me the honour to read to the Academy of Sciences, has become a source of misunderstanding and errors, and in its result has injured rather than served the cause of philosophic truth. Nevertheless I do not know how to explain this point and re-establish matters in their right position without having the appearance of complaining in some degree of MM. Nobili and Antinori; than which there cannot be to me a more disagreeable thing. I honour those gentlemen for all that they have done, not only for electricity but also for science in general, and if it were not that the contents and wording of their mémoires oblige me to speak and place me in the alternative of admitting or denying the correctness of their assertions, I should have put aside the scientific errors which I believe them to contain, leaving to others the care of removing them. These philosophers unfortunately had no other knowledge of my researches than the short letter which I wrote to M. Hachette, and not being careful to refer to my papers (though it appears to me they should have done so under the circumstances), they have mistaken altogether the sense of a phrase relating to the beautiful observations of M. Arago; they have presumed that I had not previously done that which they thought they had done themselves; and finally, they advance what appear to me to be erroneous ideas of magneto-electric currents and give their ideas as corrections of mine, which had not as yet come under their eyes.

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[1] According to the account of the sitting which is given in the Lycée, No. 35.

First let me rectify that which I consider as the most serious error, the misinterpretation given of my words; for those committed in the experiments would have been easily removed by the course of time.

MM. Nobili and Antinori say (Annales, t. xlviii. p. 428), "Mr. Faraday considers Arago's magnetism of rotation as al-"together connected with the phenomenon which he discovered "ten years ago. *He then recognised, as the notice says, that by* "*the rotation of a metallic disc under the influence of a magnet* "*we may produce electric currents in the direction of the radii* "*of the disc in sufficient quantity to make this disc become a* "*new electric machine.* We are entirely ignorant how Mr. "Faraday had ascertained this fact, and we do not know how "a result of this nature *could remain generally unknown for* "*so* LONG A TIME, *and so to say* FORGOTTEN in the hands of the "author of the discovery; besides, &c."

Now I never said that which MM. Nobili and Antinori here impute to me. In my letter to M. Hachette, referred to at the beginning of this letter, I gave a brief account of that which I had recently discovered and read on the 24th of the preceding month to the Royal Society. This notice is found page 402 of the same Number of the Annales, and it there says: "The "fourth part of the papers considers the equally curious and "extraordinary experiment of M. Arago, which, as is known, "consists in revolving a metallic disc under the influence of a "magnet. Mr. Faraday considers the phenomena exhibited in "this experiment as *intimately connected* with that of the magnetic rotation which he had the good fortune to discover ten "years ago. *He has ascertained that by the rotation* of a "metallic disc under the influence of a magnet *we may* form "electric currents in the direction of the radii of the disc in "sufficient number to make the disc become a new electric machine."

I have never said, nor ever had the intention of saying, that I had obtained electric currents by the rotation of a metallic disc at a period previous to the date of the memoir which I had then just written; but I said that the extraordinary effect discovered by M. Arago was connected in its nature with the electro-magnetic rotation which I had discovered several years before, for both of them are due to a tangential action; and I said that by the rotation of a disc near a magnet I can (now) cause electric currents to pass or tend to pass in the direction of the radii, thus constituting the disc a new electric machine: and that I think is satisfactorily proved in the part of the paper of which I was giving an account, as may be seen at pp. 65–118 of vol. l. of the Annales.

I have the most earnest desire to have this error removed, for I have always admired the prudence and philosophic reserve shown by M. Arago in resisting the temptation to give a theory of the effect he had discovered so long as he could not devise one which was perfect in its application, and in refusing his assent to the imperfect theories of others. Admiring his reserve I adopted it in this respect, and perhaps for that reason had my eyes open to recognise the truth when it was presented.

I have now arrived at that which concerns the philosophy of my writings. My paper of 24th November, 1831, contains in its fourth part my opinion of the cause of Arago's phenomenon, an opinion which I *at this time* see no reason to change. MM. Nobili and Antinori, in their writings of the 31st January and

24th March, 1832, profess to remove certain errors from among my facts, and to give extensive developments of electro-magnetic phenomena. I have not been able to perceive that the writings of these philosophers add a single fact to those contained in my papers, except it may be that they make mention of the spark obtained from the ordinary magnet, a result which I had myself obtained before from the electro-magnet. On the other hand I think that the mémoires of these gentlemen contain erroneous ideas of the nature of magneto-electric currents, and that they are mistaken both as to the action and the direction of the currents in the revolving disc of Arago. These philosophers say, "*We have recently verified, extended, and perhaps* "*rectified in some parts the results of the English philoso-* "*pher, &c.*" (Annales, l. 281.) And afterwards at p. 298, in reference to what they *supposed* to be my ideas (for though they had been read, and are now published, they had not thought proper to consult them), they say, "We have already "given our opinion on this idea; but if, at the commencement "of our researches, it appeared to us not easy to make it ac- "cord with the nature of the currents discovered by Mr. Fara- "day himself, what shall we now say after all the new obser- "vations which we have arrived at during the continuation "of our researches? We say that we have in the galvano- "meter the competent judge, and that it is for it to resolve the "question."

With the greatest desire to be corrected when in error, it is still impossible for me to discover in the writings of these gentlemen any correction by which I can profit; but I fully admit the competence of the galvanometer, and shall proceed as briefly as possible to submit to its judgement our different notions concerning the phenomenon of Arago: and I am so satisfied at present with the facts and results contained in the papers which I have published (though I could make changes in certain parts if I had to rewrite them) that I shall have no need to go beyond the experiments which they contain.

It is not my intention to enlarge further on the first mémoire of the learned Italians. I have added correcting notes to an English translation of it which has appeared in the Philosophical Magazine[1], and I have had the honour to send some copies to you and to the authors. At present my object is to compare the second part of their writings with the fourth part of my first paper and with some other portions of other papers throwing light on the general principles. The two writings have for their object the explication of the phenomenon of Arago, and fortunately both are found in the fiftieth volume of the Annales, so that reference to them is easy. I shall refer to my paper by the numbers thus indicated (F. 114.) and to the writings of MM. Nobili and Antinori simply by indication of the page of the Annales.

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[1] See page 164 of the present volume.

At page 281, after some general remarks, we read, "We "have recently verified, extended, perhaps rectified in some "parts the results of the English philosopher: *we then said* "that the magnetism of rotation found a true point of bearing "in the new facts of Mr. Faraday, and that consequently the "theory of magnetism *appeared to us* at present so far ad"vanced as to deserve that we should undertake to develope "the physical principles on which it depends. *The writing* "*which* WE *now cause to appear is destined to fill this void,* "*&c.*" On this point I will only remark, that just four months before, the paper which I had read to the Royal Society said the same thing, and had given that which is, I hope, a true and exact exposition of the philosophy of the effect in question (F. 4. 80.).

At page 282 we read, "We have already distinguished "these currents in our first researches," that is to say, in the first paper which was inserted in the Number for December (p. 412): but I had already described these currents four months before (F. 90.).

At page 283 are found described "the explorers or galva"nometric sounds," which are nothing else than what I had before described and distinguished by the name of *collectors* or *conductors* (F. 86, &c.).

At the commencement of the investigation by the Italian philosophers of the state of Arago's disc revolving in the neighbourhood of a magnet, two relative positions of the plate and magnet were chosen, one called (p. 284) the "*central arrangement*," where the magnetic pole was placed vertically over the centre of the disc, the other (p. 285), "*excentric arrangement*," in which the magnet acted out of that position. In respect of the central arrangement we read (p. 284), " in this case when " the magnet acts on the centre of the disc the sounds(collectors) " transmit no signs of a current to the galvanometer *wherever* " *they are placed*, and if small deviations are accidentally ob- " served it is only because of a fault in the centralization, so that " we have only to *correct this fault*, and immediately all signs " from this equivocal source disappear, &c. In fact, what happens " with an electro-dynamic spiral turning on its own axis always " in front of the same magnetic pole? *Absolutely nothing.* Its " turning is an *indifferent circumstance.* The formation of the " currents *belongs to an entirely different condition*, for they " are *not produced* except at the moment the spirals are *ap-* " *proached to* or *withdrawn from* the magnet. As long as the " spirals remain at a constant distance, *moving or not moving*, " there is *no current*, just as in the same manner *there is none* " in the case of central rotation, when the points of the disc, " remaining constantly at the same distance from the magnetic " pole, thus renew the combination of continual presence to " which the *new laws* of the currents of Mr. Faraday ASSIGN " *no effect*."

This statement is so erroneous in all its parts that I have been obliged to copy the whole of it. In the first place the electric currents *tend as strongly* to be produced in the revolving disc in the case of the " central arrangement " as in any other case (F. 149–156.), but their direction is from the centre to the circumference or *vice versâ*, and it is at these parts that the collectors should be applied. It is precisely this case which makes the revolving disc a new electric machine (F. 154.), and it is on this point that MM. Nobili and Antinori have altogether deceived themselves in both their mémoires. This error is found in every part of the mémoire which I am now comparing with my first paper; and appears, as I think, in all the parts, without exception, of the theory given of Arago's phenomenon in that mémoire.

It is said at p. 284, that *absolutely nothing* happens when a helix revolves on its axis concentric with a magnetic pole, and that the circumstance of rotation is indifferent. I venture to say, though I have not made the experiment, that an electric current will tend to pass transversely to the helix, and that the circumstance of rotation, instead of being indifferent, contains in this case the only essential condition required to produce currents. The helix in fact may be considered as analogous to a cylinder occupying its place, except that it is by no means so good, because it is as it were cut into a long spiral wire. The helix may be considered as a simple wire placed in any part of the space occupied by the cylinder; and I have demonstrated that such wires produce currents when they rotate, their opposed extremities being applied to a galvanometer.

It is said at p. 284, that the formation of currents "depends "upon an *entirely different condition*, for they are not pro"duced except at the moment when the spirals are either ap"proached to or withdrawn from the magnet: as long as "the spirals remain at a constant distance *there is no current* "whether they move or not, just as in the same manner there "is no current in the case of *central rotation*, &c."

Now, in my first paper I proved that the essential condition was, not an approximation or a recession, but simply that the moving metal should cut the magnetic curves (F. 101. 116. 118. &c.), and that consequently, all other things being equal, motion without change of distance is the most effectual and the most powerful means of obtaining the current, instead of being that condition in which *absolutely nothing* occurs. In my second paper *I proved* that motion across the magnetic curves was the *only condition* necessary (F. 217.), and that far from approximation or recession being required, we might produce the currents from the magnet itself, drawing them off in the proper direction (F. 220.).

Finally, when speaking of this "central arrangement," and the supposed absence of effect when "the parts of the disc "remained constantly at the same distance from the magnetic "pole," MM. Nobili and Antinori say, (p. 285) "in renewing "thus the combination of continued presence to which the new "laws of the currents of Mr. Faraday assign no effect," and then we read in a note, "these laws may be reduced to three;" which are then specified, first fully and afterwards as follows: "1st Law. During the approximation: a current produced "contrary to the producing current; repulsion between the two "systems. 2nd Law. Invariable distance: no effect. 3rd Law. "During recession: current reproduced in the same direction as "the producing current; attraction between the two systems."

I have not myself ever given the above as the simple laws which govern the production of the currents I was so fortunate as to discover; neither can I comprehend why MM. Nobili and Antinori say they are *my* LAWS, though at p. 282, one of them is so called. But I described three similar cases both in my first paper (F. 26. 39. 53.) and in the notice, *i.e.* in my letter to M. Hachette, as the general effects that I had observed. It has been shown by that which I have already said, that they are not the laws of magneto-electric action, for the simple fact of obtaining electric currents by means of the revolution of a cylinder (F. 219.), or of a disc (F. 218.) attached to a magnet, or of the magnet itself (F. 220.), contradicts every one of these laws. ONE LAW which includes the whole of the effects is given in my paper (F. 114. 116. &c.), and it simply expresses the *direction* in which the moving conducting body *intersects* the magnetic curves. This law of direction being given I endeavoured to express the whole generally (F. 118.) in the terms which I will here repeat: " All these results show " that the power of inducing electric currents is circumferen- " tially exerted by a magnetic resultant or axis of power, just " as circumferential magnetism is dependent upon and is ex- " hibited by an electric current."

I have quoted at length the passage of the learned Italians because it contains nearly all the difference between us, as to the facts and the views of this part of our subject. Having shown the errors which the passage contains, I may now be allowed to be more concise in showing by *the aid of the galvanometer* the mistakes which flowing from them, are found spread through the other parts of the mémoires. It is in fact very curious to observe how, with galvanometric indications generally correct, these philosophers have suffered themselves to be led away under the influence of preconceived notions. For example, at pp. 287, 288, and in fig. 2, plate III. we find the result of an examination by the galvanometer of the currents in a revolving disc; these currents are represented almost with perfect accuracy by means of the arrows, nevertheless the *two conclusions* which are drawn from them accords with the theory announced, but are diametrically opposed to the facts.

" One of these conclusions (p. 287) results from the imme- " diate inspection of the arrows indicating the currents in the two

" parts of the disc (fig. 2), and it is, that *in the parts* (*or side*)
" *which approach* (*or enter*), *the system of currents developed*
" *is contrary to that produced on the other side.* The other
" conclusion is obtained by comparing the currents produced
" on the disc with the currents of the producing cause; and it
" is, that, *the direction of the currents on the parts of the disc*
" *which enter* (*or approach*) *is contrary to that of the pro-*
" *ducing currents, whilst on the other side there is identity of*
" *direction in the two systems.*"

Now I had demonstrated in my first paper (F. 119.) " that " when a piece of metal passed either before a single pole, or " between the two opposite poles of a magnet, or near electro- " magnetic poles, whether ferruginous or not, electric currents " are produced across the metal transverse to the direction of " motion." This fact is proved by means of wires (F. 109.), plates (F. 101.), and discs (F. 92, &c.), and in all these cases the electric current was *in the same direction*, whether the metal approximated or receded from the magnet, provided the direction of the motion did not change. In the revolving disc of Arago the electricity which in innumerable experiments I drew from its various parts, always accorded with this result (F. 92. 95. 96.); and consequently (F. 119, &c.) I have recapitulated them in a short description as those presented by Arago's disc; establishing above all (F. 123.) that the currents produced near to or under the poles " are discharged or return in the parts of " the plate on *each* side of and more distant from the place of " the pole, where of course the magnetic induction is weaker."

I have represented this state of things under a general form in the figure 2, plate V. (joined to this paper), which, as respects the arrows, the designation of parts, &c. I have also made to correspond as well as I could with the figure 2, plate III. of the mémoire of the Italian philosophers. I will now proceed to show how far it accords with their galvanometrical results and how far with their *conclusions.*

As regards the galvanometric results, my figure might be used in place of theirs without causing any difference, and I indeed have no reason to say that their results are inaccurate.

With regard to that " one of the conclusions which result " from the immediate inspection of the arrows indicating the " currents in the two parts of the disc," or of any other attentive and experimental examination, it is seen that the entering currents *n, n, n,* instead of being in *a contrary direction* to those in the retreating parts *s, s, s,* follow exactly the same direction; that is to say, that as to the general motion, near the pole they go from above downwards, or from the circumference towards the centre transversely to the lines which the different parts describe in their course, and that at a greater distance (F. 92.) on each side of the pole they pass in the contrary direction. As any point in a line described by the motion approaches the pole, a current begins to traverse it, and increases in intensity until the point has arrived at the shortest distance (or perhaps a little beyond, if time enters as an element into the effect); afterwards, because of increase of distance, the current diminishes in intensity, but without ever having changed its direction relative to its own course. It is only when it arrives at parts still more distant where the excited electricity may be discharged, that a current appears either in the opposite direction, or more or less oblique. I presume that it is quite unnecessary to speak of the partial change in the direction of the current at the parts of the disc towards the centre or the circumference; the two or three curves which I have roughly traced will show in what direction these changes take place.

The second conclusion which results from the mémoire of the learned Italians (p. 288) is that "in the parts which approach "the direction of the currents is contrary to that of the pro"ducing currents" (that is, of those which are considered as existing in the magnet), "whilst on the other side there is "identity in the direction of these two systems." This assertion is exactly the reverse of the truth (F. 117.). By means of the arrows, fig. 2 and 1, I have indicated the direction of the currents in the magnetic pole, and it is the same as the direction given by MM. Nobili and Antinori in their figure 1, plate III. But my figure 2, as well as the indication of the galvanometer, evidently prove the approaching parts *n, n, n,* have currents which pass across them in the *same* direction as the current in that side of the magnetic pole, and that the parts which recede *s, s, s,* have currents which follow a *contrary* direction to those which are assumed as existing in that side of the magnetic pole from which they recede.

I suppose, but am not quite sure, that MM. Nobili and Antinori imagine that circular currents are excited in the part of the metal near to the pole, in the same manner and absolutely like those which are formed in a helix when it is brought towards a magnet; and that as this part of the disc recedes, the circular currents are somehow reversed, as is the case with the helix when it is withdrawn from the magnet. A passage in their first paper, and another at the end of page 284, appears to imply that such is their notion. This idea occurred to me more than a year ago, but I soon saw from many experiments (I have just quoted some of them) that it did not satisfy the facts: and when I found that the action of the helix, in approaching to and receding from the pole, was perfectly explained (F. 42.) by the law (F. 114.), I was constrained to give up my previous opinion.

The mémoire then goes on (p. 288) to explain the phenomena of Arago's revolving disc: but as I have shown that the theory generally is founded on two conclusions, which are the reverse of reality, it will not be necessary to make a close examination of this part. It is not possible that it can accurately represent the phenomena. Those who are curious to know the true state of things can decide for themselves, by the assistance of a very few experiments, whether the view which I published in the paper which first announced the discovery of these currents is the true one, or whether the learned Italians have reason to say I was in error, and have themselves published more correct views of the subject.

All the world knows that when M. Arago published his remarkable discovery, he said that the action of the disc on the magnet might be resolved into three forces: the *first* perpendicular to the revolving disc, and this he found repulsive: the *second* horizontal and perpendicular to the vertical plane containing the radius beneath the magnetic pole; this is a tangential force, and causes the rotation of the pole with the metal: the *third* is horizontal and parallel to the same radius; this at a certain distance from the circumference is null, nearer the centre it tends to urge the pole towards the centre, and nearer the circumference it tends to move it from the centre.

At p. 289, MM. Nobili and Antinori give the explanation of the first of these forces. As I have said, these philosophers consider the approaching parts of the disc as having currents contrary to those which exist in that side of the pole which they are approaching, and consequently repulsive; and they consider the parts which are receding as having currents identical in their direction with those in the side of the magnet from which they recede, and consequently these parts are attractive. The amount of each of these two forces is equal the one to the other, but as concerns the needle or magnet their relation is not the same; "the repulsive forces being the nearest exist "in the disc to the part even under the needle, and thus ob-"tain a preponderance over the action of the contrary forces "which are exerted more obliquely and further off; on the "whole, it is only one part of the repulsive force which is "balanced by the attractive force; the difference finds no "opposition, and it is that portion which produces the effect."

But I have proved in this letter that the currents in the parts which either approach or recede are exactly the reverse of those supposed by the learned Italians; and that consequently where they expect attraction they should have repulsion, and for repulsion attraction; so that according to their conclusions, corrected by experiment, the result should be *attraction* instead of *repulsion*. But M. Arago was right in saying the force was repulsive, consequently the theory of the effect here given cannot be true.

In my first paper will be found my view of this effect. I have there inquired whether it was possible or probable (F. 125.) that time may be required for the development of the maximum current in the plate, in which case the resultant of all the forces would be in advance of the magnet when the plate is rotated, or in the rear of the latter when the magnet is rotated: the line joining this resultant with the pole would be oblique to the plane of rotation: and then the force directed according to this line may be resolved into two others, the one parallel and the other perpendicular to the plane of rotation: the latter would be a repulsive force, producing an effect analogous to that remarked by M. Arago.

The *second* force is that which makes the magnet and disc mutually follow each other. On referring to page 290, and fig. 1 or 2 (my figure 2 will answer the same purpose), we read "there exist in *s*, *s*, *s*, attractive forces towards which it (the

" magnet) is drawn, and at *n, n, n* there are repulsive forces " which push it in the same direction," consequently the magnet moves after the metal. But the currents, and consequently the forces, are exactly the reverse of that which is supposed, as I have just shown: the magnet and disc therefore ought to move in opposite directions if the forces act in the manner assumed; nevertheless as the fact is they do not move in opposite directions, it is evident that the theory which explains their motions by reversing the facts must itself be erroneous.

The *third* force is that which tends to carry the magnetic pole either towards the centre or towards the circumference on each side of a neutral point in that radius above which the magnet is placed; this effect is described at p. 281, and also in the figure 4 which accompanies the mémoire, which latter I believe to be quite correct. The mémoire goes on to explain this effect by referring to the repulsive force admitted (p. 289), to render a reason for the *first* effect observed by M. Arago, namely, the vertical repulsion from the disc; and assuming that this repulsive force is spread over a certain extent of the disc under the magnet, it is concluded (p. 292, fig. 5) that if the pole is situated very near to the circumference, the portion of the disc from whence this force emanates is diminished, being cut off by the circumference itself, and consequently the parts nearer to the centre act more powerfully and push the pole outwards: whilst on the other hand, if the pole is placed nearer to the centre, the extent of disc from whence the force emanates will reach beyond the centre, and as this part beyond is considered (though wrongly) as inactive, so the portion near the circumference is the most powerful and pushes the pole towards the centre.

One or two little objections offer themselves at once to this opinion, but they are as nothing in comparison with that which arises when we remember that, according to the views of the authors themselves respecting the action of these currents, the error made in giving the direction of those excited near the pole obliges us to substitute *attraction* for *repulsion*, as I have shown in speaking of the first of these forces; consequently all the motions connected with the *third* force ought to be exactly the contrary of what they really are: and the theory which when it is corrected by experiments made with the galvanometer indicates such motions deserves to be abandoned.

At page 292 I find that the mémoire refers to the "second law" of Mr. Faraday. As I have said, I never gave the three statements as laws. In fact, I regret very much that a letter which was not intended to give minute details, but only certain facts gathered in haste from the hundreds previously described in the paper read to the Royal Society, I regret, I say, that this letter, which was never intended for printing, should have led the learned Italians into error. And yet after a re-examination of all the facts I cannot see that I am in the least degree answerable for the mistakes into which they have fallen; either as having advanced erroneous results, or as concerns the paper, in not having given to the scientific world full details as soon as it was possible for me to do so.

I have not as yet published my view of the cause of the *third* force distinguished by M. Arago; but as the Italian philosophers, when giving the hypothesis which I have just now condemned as inaccurate, say (293) "in fact what other hypo-" thesis can reconcile the *verticality* which the needle preserves " in the two positions $n\ s$, $n''\ s''$ (fig. 4) with the other fact of " repulsion from below upwards, which raises the needle in the " second position s'', n''?"—I am tempted to offer another in this place; premising always that the directions and forms which I may trace as those of the excited electro-magnetic currents are to be considered only as general approximations.

If a piece of metal sufficiently large to contain without derangement all the currents which may be excited in its mass by a magnetic pole placed above it, moves in a rectilinear direction beneath the pole, then an electric current will pass across the line of motion in all those parts in the immediate neighbourhood of the pole and will return in the opposite direction on each side in those parts which being further from the pole are subjected to a feebler inductive force, and thus the current will be completed or discharged (fig. 3). Let A, B, C, D, represent a plate of copper moving in the direction of the arrow E, and N the north end of a magnet placed above it, electric currents will be produced in the metal; and though they extend without doubt from the part just under the pole to a great distance round (F. 92.) and at the same time diminish in intensity and change in direction as the distance from this part increases, nevertheless the two circles may serve to represent the resultant of these currents: and it will be evident that the most intense point of action is where these circles touch and immediately beneath the magnetic pole; or, because of the time required, a little in advance. Hence that portion of the force which acts parallel to the plane of the metal will carry the pole in advance in the direction of the arrow E, because the forces are equally powerful on the side A, B, of the pole as on the side C, D: and that portion of the force which, because of the time necessary for the production of the excited current, is perpendicular to the direction of the metal, as I have already said, will be repulsive, and tend to push the pole upwards or outwards.

But suppose that instead of this plate which moves in a rectilinear direction, we substitute a circular disc revolving on its axis, and then consider the case of the magnetic pole placed over its centre (fig. 4), there will then be no electric currents produced, not because they do not tend to be produced, for I have already said in this letter and shown in my papers (F. 149. 156. 217.) that from the moment the disc moves, the currents also are ready to move, tending to be formed in the direction of radii from the circumference to the centre; but because all the parts are equally influenced, all of them being equally distant from the centre, so none of them can gain an excess of power over the others, no discharge can take place, and consequently no current can be formed. As no *current* can exist, so none of the effects due to the action of a current on a pole can be produced, and thus it is that there is neither *revolution* nor repulsion of the magnet. Hence the cause of the *verticality without repulsion* which occurs at this place.

Now let us consider the case where the pole of the magnet, instead of being placed over the centre of the revolving metal, is on one side, as at N, fig. 5. The tendency to the formation of electric currents is due to the motion of the parts of the disc *across* the magnetic curves (F. 116. 217.), and when these curves are of equal intensity the electric currents increase in force in proportion to the increased velocity with which the parts of the disc bisecting these magnetic curves move (F. 258.). Let us therefore trace a circle, *a b*, fig. 5, round the magnetic pole as a centre, and it will represent the projection on the disc of magnetic curves having *equal intensity*; *a* and *b* will be those points in the radius passing immediately under the pole, which are at an equal distance from the pole; but as the part *a* passes under the pole with a much greater velocity than the part *b*, the intensity of the electric current excited in that part is proportionally greater. The same is true for points in any other radius cutting the circle *a b*, and it will be also true for any other circle drawn round N as a centre, and representing therefore magnetic curves of equal intensity, except that when this circle extends beyond the centre *c* of the revolving disc, as at *c d*, instead of a weaker current at *d* than at *c* it will be a contrary current that tends to be produced.

The natural consequence of these actions of the various parts is, that, as the sum of the forces tending to produce an electric current in the direction from *c* to *d* is greater on the side *c* of the magnetic pole than on the side *d*, the curvature, or the return of these currents by the right and left, will also commence on this side, and therefore the two circles which we may regard, as before, as representing the resultants of these currents will not touch exactly under the pole, but at a greater or smaller distance from it towards the circumference, as in fig. 6.

This circumstance alone would give rise to no motion of the pole constrained so as to move only in the direction of the radius, but being combined with that which results from the *time* necessary for the development of the current, and to which I have already referred as explaining the *first* of the three forces by which M. Arago represents the action of the pole and the revolving disc, it will explain I hope perfectly all the effects which we are examining and prove also the influence of time as an element:—for let *c*, fig. 7, be the centre of the revolving disc, and *r c* a part of the radius under the magnetic pole *p*: the contact of the two circles (representing the currents) is, as we have just seen, on the side of the pole furthest from the centre *c*: but because of the element of time and the direction of the rotation R of the disc of metal, it is also a little to the left of the radius *r c*, so that the pole is subjected, not symmetrically, but obliquely to the action of these two sets of currents. The necessary consequence is that if it be free to move in the direction of the radius, and in that only, it will move towards the centre *c*, because the currents produced by a marked (or north) pole are precisely such as by their mutual action with the pole would push it in that direction.

This relation of the currents to the pole which produces them, is as easily proved by experiment as by calculation. I have shown (F. 100.) that when a marked (north) pole is above a disc revolving in the direction of the arrow R in the figures of the mémoire of the Italian philosophers or in mine, the currents (indicated by the circles) are as in fig. 3, 6 or 7. Upon bending a metal wire carrying a current in this double direction, fig. 8, and placing a marked (north) pole above it, limited so that it could only move parallel to *r c*, I found that whenever it was placed in the line *r c* it had not the least tendency to move. There is also another line perpendicular to this first line, and which crosses the contact of the circles, in which the pole has no tendency to move. But placed in any point out of these two lines it will move in one direction or the other, and when it is placed in the positions marked 1, 2, 3, 4, it will move in the direction of the arrows placed in these points. Now the position of the pole to the currents produced in the disc of M. Arago, when the magnet and disc are arranged as in fig. 5 or 7, is exactly that of position 1 in fig. 8, and hence the pole has a tendency towards the centre C.

Let us now direct our attention to that which will occur if we gradually carry the pole from the centre towards the circumference. Let fig. 9 represent the new condition of things at a given time, as fig. 5 represented the former state: it is evident that the velocities of the parts *b a* of the radius under the pole do not differ so much from each other as they did before, being now nearly as 1 : 1½, instead of 1 : 6, and with all the magnetic curves of equal intensity comprised within this circle the difference will be even less. That alone would cause that the place of the magnetic pole and the place of contact of the circles which represent the currents (fig. 7) would approach the one to the other in the direction of the line *r c*, and consequently carry the pole at 1, fig. 8, nearer to the neutral line *l i*. Casting the eyes on the second circle *d*, fig. 9, of magnetic curves of equal intensity, we perceive that as the disc does not extend to *c*, or even beyond *a*, there is nothing to add to the force of the current on that side of the pole, whilst at *d*, the radius moving across magnetic curves adds to the intensity of the current excited in *b* and everywhere else on this side of the pole, and can easily, according to the position of the pole over the plate of metal (that is, nearer to or further from the edge), render their sum equal to or greater than the sum of the forces on the other side, or towards the circumference. If the sums of the forces on the two sides of the pole are equal, then the pole will be in some part of the neutral line *l i*, as in 5, fig. 8, and will have no tendency either towards the centre or the circumference, though its tendency to move with the disc or upwards from the disc will remain unchanged. Or if the sum of the forces is greater on the side *d* than on the side *c*, then the pole will be in the position 2, fig. 8, and will be urged outwards in the direction of the radius, in conformity with Arago's results.

Besides this cause of change in the motion of the pole parallel to the radius, and which depends on the position of the pole near the circumference, there is another cause which occurs, I believe, at the same time and assists the action of the former. When the pole is placed near the edge of the disc the discharge of the currents excited near the centre is resisted at the part towards the edge in consequence of the want of conducting matter: so that instead of having the regular forms represented in figs. 7 and 8, they will, as in fig. 10, be arrested and directed in their course towards the circumference, whilst they will have all the room necessary for their motion in those places where they are directed towards the centre. That alone would cause that the point of greatest force would be a little nearer the centre than the projection of the axis of the magnetic pole, and would assist in placing the pole in the position 2, fig. 8. I have such confidence in this opinion, that though I have not had the opportunity of making the experiment myself, I venture to predict, that if instead of using a revolving disc, a band or plate of metal 5 or 6 inches wide, as A, B, C, D, fig. 11, were moved in a rectilinear direction according to the arrow under a magnetic pole placed at *a*, the pole would tend to move forward with the plate as before, but not to the right or left; whereas if the pole were placed above the point *b*, it would also tend towards the border A B; or if it were placed over *c* it would tend to move towards the border C D.

Having thus replied to the question of "What other hypotheses," &c. put by the authors of the mémoire at p. 293, I may now continue the examination of the mémoire. At p. 294 the error relative to the nature of the currents (*i. e.* their supposed inversion) is repeated: such inversion is the case with a helix and some other forms of apparatus; but the simple and elementary current produced by the motion of a wire before a magnetic pole is not reversed as the wire recedes (F. 171. 111. 92.).

At page 295 it is supposed that when the rotation is slow "the revolution of the currents is circumscribed in small limits, "and there is *little to add* to the results which have served as a "foundation to the whole of the (our) theory." But when the motion is rapid the currents envelope the whole disc, "so as to become a kind of labyrinth." For my part, I believe that the currents have the same general direction which I have given already in the figures, whether the rotation be slow or rapid, and that the only difference is an augmentation of force with increase of velocity.

A condition is then chosen (in the mémoire), really simple, though it appears at first complicated, that, where the opposite poles are placed over a plate so as to be in the same diameter, but on opposite sides of the centre. This condition, with the direction of revolution, and the currents produced, is found in fig. 7 of the mémoire of the Italian philosophers. It is not necessary to quote pp. 296, 297, which explains this figure, but I will give my figure 12, which accords with my views and experiments, and which so far corresponds with the former figure that the two may be compared with each other. It is very satisfactory to me to find that in this part of the mémoire, as well as in the first, I do not find a single *important* experimental result adverse to the views which I have published, though I am very far from adopting the conclusions drawn from them.

If we examine fig. 12 we shall see that it results in the simplest possible manner from the use of two contrary poles; thus as to the upper or north pole only, the currents are as in fig. 6. But as with this pole the current produced by it goes from the circumference towards the centre, so with the south pole in the same or a corresponding position the currents will go from the centre to the circumference (F. 100.), and consequently in fig. 12 they will continue along the diameter N S, across the centre of the revolving plate, to return in the direction of the arrows at the sides E O. The points where I do not agree with the *indications of the galvanometer* obtained by MM. Nobili and Antinori are, first, the direction of the currents in N and in S, which with them are contrary to those which I obtain; and secondly, the existence of an oblique axis of power, as at P Q of their figure 7.

The mémoire finishes, as far as I am concerned, at p. 298, in again speaking of the error (but not as an error) relative to the revolving disc becoming a *new electrical machine*. At the commencement the authors, but little acquainted with the principles under the influence of which such a result is obtained, deny it; and though they here say further, "What shall we say "after all the *new observations* which we have made during "the continuation of our researches?" I am still in no degree moved to alter anything that I have published: on the contrary, I have more confidence than before in it; since if their conclusions had been in accordance with the results I had arrived at, I should have had great reason, after the examination I have just made, to fear that my own views were erroneous.

I cannot terminate this letter without again expressing the regret I feel in having been obliged to write it: but if it be remembered that the mémoires of the Italian philosophers were written and published *after my* original papers; that their last writing has appeared in the same Number of the *Annales de Chimie et de Physique* with mine; and that consequently they have the *appearance* of carrying science beyond that which I had myself done; that both their papers accuse me of errors in experiment and theory and, beyond that, of good faith; that the last of these writings bears the date of March, and has not been followed by any correction or retraction on the part of the authors, though we are now in December; and that I sent them several months ago (at the time when I sent to you and other persons) copies of my original papers, and also copies of notes on a translation of their first paper[1]; and if it be remembered that after all I have none of those errors to answer for with which they reproach me; and that the mémoires of these gentlemen are so worded that I was constrained to reply to the objections they made against me; I hope that no person will say that I have been too hasty to write that which might have been avoided; or that I should have shown my respect for the truth or rendered justice to my own writings and this branch of science, if, knowing of such important errors, I had not pointed them out.

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[1] See page 164, &c.

I am, my dear Sir, yours very faithfully,
M. Faraday.

New Experiments relative to the Action of Magnetism on Electro-dynamic Spirals, and a Description of a new Electro-motive Battery. By Signor Salvatore Dal Negro; *with Notes by* Michael Faraday, *F.R.S.*[1]

[*Addressed to* Dr. Ambrogio Fusinieri, *Director of the Annali delle Scienze, &c. &c.*]

Sir,

On repeating the experiments relative to the action of terrestrial magnetism on electro-dynamic spirals, an action which was first observed[2] by the two illustrious Italian philosophers Nobili and Antinori, it occurred to me to examine the effect of an ordinary magnet on similar spirals at the moment when one of the poles traversed the axis of the spiral (*Exp. Res.* 39. 41. 114.), and I obtained such results as indicated the path which it would be proper for me to follow, in order to profit by this new property of magnetism. Ultimately I succeeded in constructing a new electrometer, by means of which the efficacy of the instantaneous currents discovered by the celebrated Faraday may be augmented without limit, and obtained in succession with such celerity as to render (as it were) continual the action of these currents[1]. He [Dr. Fusinieri] has already witnessed the principal part of these my experiments, and more than once has been so good as to assist me faithfully in registering the results, and has solicited a description that might be made public. I did not hesitate to make a brief exposition that might be transmitted and inserted in the forthcoming number of his Journal. He returned from us as quickly as possible, and did not forget to take with him the magnet I had promised.

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[1] Lond. and Edinb. Phil. Mag., 1832, vol. i. p. 45. That date which at the top of these pages is on the left-hand is the date of the Italian paper, that on the right-hand is the date of my notes.—M. F.

[2] [This is an error. A long section is devoted to terrestrial magneto-electric induction in my original researches (140 to 192) of the date of December 21, 1831. As my brief letter to M. Hachette is continually taken instead of my memoirs as representing my views of magneto-electricity, I venture to add a few notes and references to this paper, in the same manner as I have done to the paper by Signori Nobili and Antinori, at page 401, of the last volume of the Phil. Mag. and Annals.—M. F.]

His most affectionate friend,

Padua, April 20, 1832. Salvatore Dal Negro.

New Experiments, &c. &c.

1. Place a cylindrical tube of paper surrounded by a spiral of silk-covered copper wire upright upon a little table, and connect the extremities of the spiral with a very sensible galvanometer, constructed according to the method of Signor Nobili: introduce the north pole of an ordinary horse-shoe magnet into the axis of the cylinder, and an electric current will be obtained, which will act strongly on the galvanometer. (*Exp. Res.* 39. 147.) On withdrawing the pole of the magnet, a current, in the contrary direction, will be obtained (*Exp. Res.* 39.). On repeating the experiment with the south pole, currents will be manifested in the contrary direction (*Exp. Res.* 114. &c.) to those caused by the north pole, and less powerful, as has been observed.

2. Introduce into the same spiral the north pole of a more powerful magnet than the first, and the conflict will produce a much greater effect; I say, "conflict," because the phenomena in question obey the laws of the collisions of solids. The magnetism of rotation discovered by the celebrated Arago has already shown what influence motion has in these phenomena. Then slowly moving the magnet, it may be introduced and removed from the spiral without causing any sensible current. To obtain the maximum effect, it is necessary that the

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[1] I have described at length a different but perfect way of obtaining a continuous current by magneto-electric induction. (*Exp. Res.* 90. 154. 155. 156. &c.)—M. F.]

magnetic pole should make its entrance or exit with great velocity. (*Exp. Res.* 136. 153. 258.)

3. Introduce at the same time the poles of the magnet into two equal spirals, having the same direction, and two contrary currents will be obtained, which would destroy each other if the poles of the magnet were of equal strength. But as the north pole is in our latitudes more active than the south, the effect obtained will equal the difference of the two currents, and be in the direction of the greater force; exactly as happens in the collision of solids. It results from this my experiment, that henceforth we may ascertain at once with facility which is the most powerful of two magnets, and how much more active the north pole is than the opposite south pole of the same magnet[1].

4. In order to take advantage at the same moment of both the poles of the same magnet, construct two spirals turning in opposite directions, and place them as usual in connection with the galvanometer. Then on introducing the poles of the magnets, an effect will be obtained, equal to the sum of those which could be produced by the poles separately. To measure the effect produced by these two spirals with a more powerful magnet than the first, I was obliged to use a galvanometer of only one-twentieth the sensibility of the first.

5. I immediately perceived that this pair of spirals was a valuable element capable of furnishing a mode of augmenting without limit the efficacy of the instantaneous currents. I

[[1] The statement that the north pole is in our latitudes more powerful than the south is a mistake. The cause of the effects obtained by Signor Negro will be found at (147) of my Exp. Research., and is dependent on the inductive force of the earth, as a magnet, upon other magnets, as well as upon soft iron. When a straight magnet is held in the dip, or even vertically with its marked pole downwards, both poles are strengthened; when held with its unmarked pole downwards, both poles are weakened. And though when a horse-shoe magnet is held with both poles downwards, as in Signor Negro's experiment, the marked pole is stronger than the unmarked one, it is only because the two limbs are affected as the single magnets just referred to, and the bend of the magnet being the upper part becomes virtually a feeble south pole. If the horse-shoe magnet be held with its poles upwards, then the contrary effect happens, and the unmarked (usually called the south) pole becomes the stronger; or if both poles are in equal relation to the magnetic dip, then both are equally strong.—M .F.]

therefore instantly constructed a second pair of spirals equal to the first, and putting both in connection with the galvanometer, I caused two magnets to enter them contemporaneously, and obtained an effect due to the sum of both pair of spirals. On using still more powerful magnets, even the second galvanometer became useless. The galvanometer which I substituted consists of a rhomboidal needle, about five Paris inches in length, and suspended as in the ordinary compass. The wire which connects the extremities of the spirals passes beneath the needle distant about 3½ lines, and is parallel to it when the latter is at rest; on obtaining this fortunate result I conceived the idea of constructing a battery of several magnets put in conflict with an equal number of pairs of spirals.

Construction of a new Electro-motive Battery.

6. I had at command only four magnets, so that for the present I am limited in my construction to four pairs of spirals, as in the manner following: On a little table is placed one after the other four pairs of spirals, with the axes horizontal, and so that the perimeters of the cylinders shall have the same horizontal line as a common tangent, it being parallel to one of the sides of the table. On a second table contiguous to the first, but not in contact, was placed a little carriage consisting of a rectangular table supported on four wheels, by means of which it could easily receive a motion to and fro. The four magnets were placed upon this carriage, so that the poles of each could move horizontally towards the pairs of spirals, and enter within them.

The magnets were firmly fixed on the carriage so as not to alter in position, and the latter was so arranged as to move to and fro only in one direction. On moving the carriage, the limbs of the magnets passed at once into all the spirals, and they could be made to enter or move out with the utmost facility, and with any required velocity.

That the battery thus disposed may give an electric current equal in force to the sum of all the currents excited in the pairs of spirals, it is necessary that all the spirals turning to the right should communicate with each other, that they may form a single metallic wire. The same must be done with all those turning to the left. Then these wires are to be connected in the usual well-known manner with a galvanometer, which we may suppose placed on a third little table, so far distant from the magnets that it may not be influenced by their presence. Although these electric currents are only obtained of instantaneous duration naturally, nevertheless with my battery they may be excited successively with such celerity as to produce an action, which is as it were continuous[1]. From the little I have done, and from what I have said, it follows that being able by this method to sum up the simultaneous action of an indefinite number of electric currents, this my battery may become fulminating.

I hope I have said enough to enable my readers to comprehend the mode of constructing this electro-motive battery. Hereafter, and by the help of a figure, I will describe the most useful and convenient distribution of the elementary pairs, and the mode of obtaining the maximum effect when employing the smallest possible number of elements, or of pairs of spirals.

On the Magneto-electric Spark and Shock, and on a peculiar Condition of Electric and Magneto-electric Induction[2].

To Richard Phillips, Esq., F.R.S., &c.

My dear Sir,

If you think well of the following facts and reasoning, you will, perhaps, favour them with a place in the Philosophical Magazine.

When I first obtained the magneto-electric spark[3], it was by the use of a secondary magnet, rendered for the time active by a principal one; and this has always, as far as I am aware, been the general arrangement. My principal was an electro-magnet; Nobili's was, I believe, an ordinary magnet; others have used the natural magnet, but in all cases the secondary magnet was a piece of soft iron.

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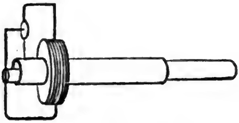
[1] [See the note at page 201.—M.F.]

[2] Lond. and Edin. Phil. Mag. 1834, vol. v. p. 349.

[3] Philosophical Transactions, 1832, p. 132. [See also Phil. Mag. and Annals, N.S. vol. xi. p. 401, &c.—Edit.]

The spark is never the electricity of the principal, or even of the secondary magnet. The power in the first induces a corresponding power in the second, and that induces a motion of the electricity in the wire round the latter, which electricity produces the spark. It seemed to me, however, no difficult matter to dispense with the secondary or temporary magnet, and thus approach a step nearer to the original one; and this was easily accomplished in the following manner. About 20 feet of silked copper wire were made into a short ring helix, on one end of a pasteboard tube, through which a cylindrical magnet, an inch in diameter, could move freely; one end of the helix wire was fastened to a small amalgamated copper plate, and the other end bent round so as to touch this plate perpendicularly upon its flat surface, and also in such a manner that when the magnet was passed through the cylinder it should come against this wire, and separate the end from contact with the plate. The consequence was that whenever this action was quickly performed, the magneto-electric spark appeared at the place of disjunction.

My apparatus was placed horizontally, and a short loose plug of wood was put into the end of the cylinder, so that the disjunction at the plate should take place at the moment the end of the magnet was passing through the helix ring, that being the most favourable condition of the apparatus. The magnet was driven with a sharp quick motion through the cylinder, its impetus being overcome, as soon as the spark was obtained, by an obstacle placed at a proper distance on the outside of the moveable wire. From the brightness and appearance of the spark, I have no doubt that if both ends of a horse-shoe magnet were employed, and a jogging motion were communicated to the light frame carrying the helices, a spark equal, if not superior, to those which down to this time have been obtained with magnets of a certain power, would be produced.



Thus the magneto-electric spark has been brought one step nearer to the exciting magnet. The much more important matter still remains to be effected of rendering that electricity which is in the magnet itself, and gives it power, evident under the form of the spark.

The next point to which I wish to direct your attention is the *magneto-electric shock*. This effect I have felt produced by Mr. William Jenkins in a manner that was new to me; and as he does not intend to work out the result any further, but has given me leave, through Mr. Newman, to make it known to you, I think the sooner it is published the better. Mr. Jenkins's apparatus consists of a helix cylinder formed of copper wire in the usual manner. An iron rod, about 2 feet long and half an inch in diameter, can be passed at pleasure into the centre of this cylinder. The helix consists of three lengths of wire, (which, however, might as well be replaced by one thick wire,) the similar ends of which are soldered to two thicker terminal wires, and on these are soldered also two short copper cylinders, to be held in the hand and give extensive contact. The electro-motor was a single pair of plates, exposing, perhaps, 3 square feet of surface on *both* sides of the zinc plate. On holding the two copper handles tightly in the hands, previously moistened with brine, and then alternately making and breaking the contact of the ends of the helix with the electro-motor, there was a considerable electric shock felt in the latter case, *i.e.* on breaking contact, provided the iron rod were in the helix; but none either on making or breaking contact when the latter was away.

This effect appears very singular at first, in consequence of its seeming to be the shock of the electricity of a single pair of plates. But in reality it is not so. The shock is not due to the electricity set in motion (through the body) by the plates, but to a current in the reverse direction, induced by the soft iron electro-magnet at the moment when, from the cessation of the original current, it loses its power. It is, however, very interesting thus to observe an original current of electricity, having a very low intensity, producing ultimately a counter (second) current having an intensity probably a hundredfold greater than its own, and the experiment constitutes one of the very few modes we have at command of converting quantity into intensity as respects electricity in currents.

It has been generally supposed that the electric spark producible by a single pair of plates can only be obtained upon breaking contact; but this, as I have shown in the Eighth Series of my Experimental Researches, is an error, and a very important one as regards the theory of voltaic electricity[1]; it is, however, true that the spark upon breaking contact can be very greatly exalted by circumstances having no such effect upon that produced at the moment of making contact.

Every experimenter on electro-magnetism is aware, that when the current from a single pair of plates is passed through a helix surrounding a piece of soft iron (to produce an electro-magnet,) the spark, upon breaking contact, is much brighter than if the soft iron were away; and because this effect occurs at the same moment with the shock in Mr. Jenkins's experiment, it might at first be supposed that the electricity producing both the spark and the shock was the same, and that the effects of both were increased, because of the increase in power of this their common cause. But the fact is not so, for the electricity producing the spark is passing in one direction, being that which the zinc plate and acid determine, whilst the electricity producing the shock is circulating in the contrary way.

From the appearance of the spark, which is always in this form of the experiment due to the electricity which is passing at the moment when contact is broken, it might seem that a greater current of electricity is circulating during the time that the contact is preserved, whilst the iron is present in the helix, than when it is away. But this is not the case; for when the quantity is measured by a very delicate galvanometer, it is found to remain unchanged after the removal or replacement of the iron, and to depend entirely upon the action at the zinc plate. Still the appearance of the spark is an evident and decisive proof, that the electricity which is passing at the moment of disjunction is of greater intensity when the iron is in the helix than when it is away, and this increased effect is evidently dependent, not upon any change in the state of things at the source of the electricity, but in a change of the condition of the conducting wire caused by the presence of the soft iron. I do not suppose that this change is *directly* connected with the magnetizing power of the current over the iron, but is due rather to the power of the iron after it becomes a magnet, to react upon the wire; and I have no doubt, though I have not had time to make the experiment, that a magnet of very hard steel, of equal force with the soft iron magnet, if put into the helix in the same direction, would exert an equal influence over the wire[1].

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[1] See this corrected, p. 5 of the preface of the first volume of these Researches and papers; and as to the whole paper see Exp. Res. 1048, &c.—M. F. Dec. 1843.

I will now notice another circumstance, which has a similar influence in increasing the intensity of the spark which occurs when the junction of the circuit is broken. If a pair of zinc and copper plates immersed in acid are connected by a short wire, and all precautions are taken to avoid sources of inaccuracy, then, as I have already shown, the spark, upon breaking contact, is not greater than that upon making contact. But if the connecting wire be much lengthened, then the spark upon breaking contact is much increased. Thus, a connecting copper wire of $\frac{1}{18}$th of an inch in diameter when 12 inches long, produced but a small spark with the same pair of plates which the moment before or after would give a large spark with a wire of the same diameter and 114 feet long. Again, 12 inches in length of wire $\frac{1}{9}$th of an inch in diameter gave a much smaller spark than 36 feet of the same wire.

In both these cases, though the long wires gave the larger spark, yet it was the short wires which conducted the greatest quantity of electricity in the given time; and that was very evident in the one of small diameter, for the short length became quite hot from the quantity of electricity passing through it, whereas the longer wire remained cold. Still there can be no doubt that the sparks from the long wires were of greater intensity than those from the short wires, for they passed over a greater interval of air; and so the paradoxical result comes forth, that currents of electricity having the same common source, and passing the same quantity of electricity in the same time, can produce in this way sparks of very different intensity.

This effect, with regard to lengthened wires, might be explained by assuming a species of momentum as being acquired by the electricity during its passage through the lengthened conductor, and it was this idea of momentum which guided

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[1] See forward, p. 211. It does not do so, and for reasons very evident when we consider how much less the magneto-inductive action is exerted upon hard steel than upon soft iron (Nov. 1843).

Signori Nobili and Antinori in their process for obtaining the magneto-electric spark by means of a common magnet. Whether a current of electricity be considered as depending upon the motion of a fluid of electricity or the passing of mere vibrations, still the essential idea of momentum might with propriety be retained. But it is evident that the similar effect produced by the soft iron of increasing the intensity of the spark cannot be explained in this way, *i. e.* by momentum; and as it does not seem likely that the effects, which in these cases are identical, should have two causes, I believe that both are produced in the same way, although the means employed are apparently so different.

When the electric current passes through a wire, that wire becomes magnetic; and although the direction of the magnetism is peculiar, and very different to that of the soft iron placed in the helix of the first experiments, yet the direction of the magnetic curves, both of the wire so magnetized and of the soft iron magnet, in relation to the course which the current is pursuing (*i. e.* in the conducting wire), is the same. If, therefore, we refer the increased spark to a peculiar effect of induction exerted by the magnetism over the passing electric current, all becomes consistent. Let us, for instance, for the sake of reference, represent the magnetism by the magnetic curves: then, in the first place, the longer the wire the greater the number of magnetic curves which can exert their inductive influence; and the effect in a wire of a hundred feet in length will be nearly a hundred times greater than in a wire of the same diameter only a foot in length. The reason why a core of soft iron produces the same effect as elongation of the wire, will be that it also brings magnetic curves into inductive action exactly in the same direction as those around the wire; and the rest of the circumstances, as far as I can perceive, will accord with the cause assumed.

That the magnetic curves of the wire carrying the current shall actually affect the character of the current which gives them origin, need not excite any difficulty, for this branch of science shows many such cases. Ampère's experiment of revolving a magnet on its own axis, and the case which I have shown of drawing away electricity from the poles and equator of a magnet when it is revolved, are both instances of the same kind.

In conclusion, I wish to say that I think I see here some of those indications of an *electro-tonic* or peculiar state, of which I have expressed expectations in the second series of my Experimental Researches, par. 242.[1]; for though I here speak of magnetism and magnetic curves for the sake of reference, yet allowing Ampère's theory of the magnet, all the effects may be viewed as effects of induction produced by electrical currents. Hence many extensions of the experiments. I have no doubt, for instance, that if a long wire were arranged so as to discharge a single pair of plates, and the spark occurring at the breaking of contact were noted, and then another wire carrying a current in the same direction from another electromotor, were placed parallel and close to but without touching the first, the spark obtained on breaking the contact at the first wire would be greater than before. This experiment can easily be made with a double helix; but at my present distance from town I have no means of trying the experiment, or of examining more closely these indications[2].

I am, my dear Sir, very truly yours,

Brighton, Oct. 17, 1834. M. FARADAY.

Additional Observations respecting the Magneto-electric Spark and Shock[3].

To Richard Phillips, Esq., F.R.S., &c.

MY DEAR SIR,

LIKE most things done in haste, my letter to you last month involves several errors, some from want of attention, others from want of knowledge. Will you do me the favour to print the present in correction of them?

The first error consists in supposing the electricity of the shock and the electricity of the spark (obtained at the moment of disjunction) are due to different currents, page 207 (of this volume). They are, as I find by careful experiments, due to the same current, namely, one produced by an inductive action at the moment when the current from the electromotor ceases.

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[1] Philosophical Transactions, 1832, p. 189.

[2] See forward, p. 211.

[3] Lond. and Edin. Phil. Mag. Dec. 1834, vol. v. p. 444.

If at p. 206, line 28, after "set in motion" be inserted "through the body;" and at line 33 for "counter" be read "second", and if the above statement be allowed to stand for that in page 207, this error will be corrected.

The experimental results which I anticipated, page 208, lines 1—5, and page 210, lines 9—15, do not occur except under peculiar circumstances, and I am now aware why, for natural reasons, they should not. All the effects, in fact, belong to the inductive action of currents of electricity described in the first section of the first series of my Experimental Researches. I have investigated them to a considerable extent, and find they lead to some exceedingly remarkable and novel consequences. I have still some points to verify, and shall then think it my duty to lay them (in continuation of my first paper) before the Royal Society[1].

I am, my dear Sir, very truly yours,

Royal Institution, Nov. 20, 1834. MICHAEL FARADAY.

Reply to Dr. John Davy's "*Remarks on certain Statements of* Mr. Faraday *contained in his* '*Researches in Electricity*[2].'"

To Richard Phillips, Esq.

MY DEAR PHILLIPS,

You know as well as most persons how great my dislike is to controversy, but you also know that upon some rare occasions I have been driven into it; an occasion of this kind constrains me at present to ask a favour of you. On the 22nd of January of this year, two papers were read at the Royal Society, the first entitled "Remarks on certain Statements of Mr. Faraday contained in the Fourth and Fifth Series of his Experimental Researches in Electricity," by Dr. Davy; the second, "A Note in reference to the preceding Observations," by myself. These the Royal Society did not think fit to publish in the Philosophical Transactions, but the notice of the readings appears in the 'Proceedings' of the Society, No. 19, and in your Philosophical Magazine, April 1835, page 301.

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[1] This paper is in the former volume as the *Ninth* Series of the Experimental Researches, p. 322.

[2] Lond. and Edinb. Phil. Mag. May 1835, vol. vii. p. 337.

See Jameson's Edinburgh New Philosophical Journal, October 1835, p. 317–325.

I now find that Dr. Davy has published his paper in the last number of the Edinburgh New Philosophical Journal, p. 317. I was in hopes that if that paper appeared in print mine might have immediately followed it; and meeting Dr. Davy in the Royal Institution in May last, asked him to do me the favour to allow that to be the case: this, I presume for good reasons, (which, however, I do not understand,) he declined. I am thus placed in a difficult position; for, however willing Professor Jameson, the learned Editor of the Edinburgh Journal, may be to act impartially, and give me the same opportunity of publication he has given Dr. Davy, he cannot do so before the lapse of three months. Under these circumstances, and with the old adage before my eyes that "delays are dangerous," may I beg you to insert this letter and my paper in the next Number of the Philosophical Magazine? and I may still, perhaps, be indebted to the kindness of Professor Jameson for its insertion in the next Number of the Journal in which Dr. Davy's "Remarks" have appeared.

I am, my dear Sir, most truly yours,

Royal Institution, Oct. 10, 1835. M. FARADAY.

The secretary of the Royal Society having mentioned to me the preceding paper, I requested a sight of it, that I might as soon as possible correct any error in the papers to which it referred, and of which it might make me conscious; and having read it, I am induced to hope the present note may accompany Dr. Davy's observations.

I do not know that I have any right to suppose Dr. Davy generally does not understand me in my papers, and yet something of this kind must have occurred; for instance, the new law of conduction referred to in my Fourth Series[1] is not even now evident to him, and therefore I think I cannot have erred in supposing Sir Humphry Davy unacquainted with it. The *law* is, that *all substances* decomposable by the pile are in the fluid state conductors, and in the solid state non-conductors, of the electricity of the voltaic battery (393. 394. 404. 407. 413. 505. 676. 679. 697., &c.[1]). The more careful examination of this law in other parts of my printed Researches shows that no bodies but electrolytes have this relation to heat and electricity, the few exceptions which seem to occur being probably only apparent (690. &c.[1]). That the title of *law*, therefore, is merited, and that this law was not known to Sir Humphry Davy, are, I think, justifiable conclusions, notwithstanding Dr. Davy's remarks. As to Priestley's results with the electric machine, they really have nothing to do with the matter.

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[1] An Abstract of Mr. Faraday's Fourth Series will be found in Lond. and Edinb. Phil. Mag., vol. iii. pp. 449, 450.—EDIT.]

I have said that Sir Humphry Davy spoke in general terms. " The mode of action by which the effects take place is stated very generally, so generally indeed that probably a dozen precise schemes of electro-chemical action might be drawn up differing essentially from each other, yet all agreeing with the statement there given (482.)." In this and other parts of what I have written (483. 484.[2]), which Dr. Davy quotes, he thinks that I have been deficient in doing justice, or in stating Sir Humphry Davy's " hypotheses" correctly.

Dr. Davy for my word " general" substitutes " vagueness". I used *general* in contradistinction to *particular*, and I fear that vagueness cannot with propriety stand in the same relation. I am sure that if Sir Humphry Davy were alive, he would approve of the word I have used; for what is the case? Nearly thirty years ago he put forth a *general* view of electro-chemical action, which, as a general view, has stood the test to this day; and I have had the high pleasure of seeing the Royal Society approve and print in its Transactions of last year, a laborious paper of mine in support and confirmation of that view (1834.

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[1] The paragraphs here referred to belong to Mr. Faraday's Fourth and Seventh Series, and will be found reprinted in Lond. and Edinb. Phil. Mag., vol. v. p. 166–169.—Edit.]

[2] These paragraphs belong to the Fifth Series, noticed in Lond. and Edinb. Phil. Mag., vol. iii. p. 460.—Edit.]

Part ii. page 448.[1] Exp. Res. Series viii.). But that it is not a particular account is shown, not merely by the manner in which Sir Humphry Davy wrote, but by the sense of his expressions, for, ***as Dr. Davy says,*** "he attached to them no undue importance, believing that our philosophical systems are very imperfect, and confident that they must change more or less with the advancement of knowledge[2];" and what have I done but helped with many others to advance what he began; to support what he founded?

That I am not the only one, as Dr. Davy seems to think, who cannot make out the precise (or, I would rather say, the particular) meaning of Sir Humphry Davy in some parts of his papers may be shown by a reference to Dr. Turner's excellent Elements of Chemistry, where, at page 167 of the fifth edition, the author says: "The views of Davy, both in his original essay and in his subsequent explanation (Philosophical Transactions 1826), were so *generally and obscurely* expressed that chemists have never fully agreed, as to some points of the doctrine, about his real meaning. *If* he meant that a particle of free oxygen or free chlorine is in a negatively excited state, then his opinion is contrary to the fact, that neither of these gases affect an electrometer," &c. &c. Having similar feelings, I thought that I was doing Sir Humphry Davy far more justice in considering his expressions as general, and not particular, except where they were evidently intended to be precise, as in the cases which I formerly quoted (483. 484.)[3].

Again, Dr. Davy says, "What can be more clear than this; that my brother did not consider water as essential to the formation of a voltaic combination?" &c. If this be so clear, how happens it that Mr. Brande, in the last edition of his Manual, vol. i. p. 97, says that "Sir Humphry Davy further remarks that *there are no fluids, except such as contain water*, which are capable of being made the medium of connexion between the metals of the voltaic apparatus;" and Mr. Brande's observation is, "This, however, appears to me to admit of doubt."? How happens it also that Dr. Ure, in giving his eloquent account of Sir Humphry Davy's discoveries[1], uses the very same words as those I have quoted from Mr. Brande, adding, "It is probable that the power of water to receive double polarities and to evolve oxygen and hydrogen is *necessary* to the constant operation of the connected battery."? I ought, perhaps, rather to ask, How could Sir Humphry Davy use such words, and mean what Dr. Davy wishes to be considered as his meaning? Why, *there can be no doubt that if I had proved that water was the only substance that could perform these duties, Dr. Davy would have claimed the discovery for his brother.*

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[1] [See Lond. and Edinb. Phil. Mag., vol. vi. p. 181.—EDIT.]

[2] Phil. Trans. 1826, p. 390. Edinb. New Phil. Journ., Oct. 1835, p. 323.

[3] I may be allowed to quote in a note a passage from one of Mr. Prideaux's papers, of the date of March 1833; I was not aware of it when I wrote in answer to Dr. Davy. Mr. Prideaux says, "Sir Humphry Davy's theory assumes that 'chemical and electrical attractions are produced by the same cause; acting in one case on particles, in the other on masses: and the same property, under different modifications, is the cause of all the phenomena exhibited by different voltaic combinations.' A view so comprehensive, embracing every modification of chemical as well as electrical action, seems to include the other two, *and every one that has been or can be attempted* on the subject. But what it gains in extent it wants in distinctness." Lond. and Edinb. Phil. Mag., vol. ii. p. 215.

As I cannot impute to Dr. Davy *the intention of doing injustice*, the only conclusion I can come to is that the language of Sir Humphry Davy is obscure even to his brother, who thinks it perfectly clear; so obscure, indeed, as to leave on his mind the conviction of a meaning the very reverse of that which it bears to Mr. Brande and Dr. Ure. Thus Dr. Davy puts his seal to the truth of Dr. Turner's observation[2] by the very act of denying it.

What makes the matter still more remarkable is, that Dr. Davy charges it upon me as a fault, that I, and *I alone*, have said what he denies in words, but proves in fact; whereas *I have not said* it, and others have.

If Sir Humphry Davy's meaning is thus obscure to his brother, I have no right to expect that mine should have been rightly taken; and therefore it is that I suspect, as I said before, that Dr. Davy generally does not understand me in my papers.

That "probably a dozen precise schemes of *electro-chemical action* might be drawn up" differing from each other, but all agreeing with Sir Humphry Davy's general statement, is no exaggeration. I have in the very paper which is the subject of Dr. Davy's remarks quoted six: 1. that of Grotthus, (481.); 2. of Sir Humphry Davy himself (482.); 3. of Riffault and Chompré (485.); 4. of Biot (486.); 5. of De la Rive (489.); and 6. my own (518. &c.). These refer to modes of decomposition only; but as I spoke in the passage above quoted of "electro-chemical action" in reference to chemical effects and their cause generally, I may now quote other particular views. Volta, Pfaff, Marianini, &c. consider the electricity of the voltaic pile due to contact alone. Davy considered it as excited by contact, but continued by chemical action. Wollaston, De la Rive, Parrot, Pouillet, &c. considered it as of purely chemical origin. Davy, I believe, considered the particles of matter as possessing an inherent electrical state to which their chemical properties were due; but I am not sure of his meaning in this respect. Berzelius, according to Turner, views them as being naturally indifferent, but having a natural appetency to assume one state in preference to another[1], and this appears to be the theory of M. Fechner also[2]. Again, electro-chemical phenomena have been hypothetically referred to vibrations by Pictet, Savary, myself, and others. Now, all these views differ one from another; and there are, I think, a dozen of them, and it is very likely that a dozen more exist in print if I knew where to look for them; yet I have no doubt that if any one of those above could be proved by a sudden discovery to be the right one, it would be included by Dr. Davy, and, as far as I can perceive, by myself also, in Sir Humphry Davy's general statement. What ground is there, therefore, for Dr. Davy's remarks on this point?

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[1] Chemical Dictionary, *art.* ELECTRICITY.

[2] And to that of Mr. Prideaux's also.

In reference to another part of Dr. Davy's observations I may remark, that I was by no means in the same relation as to scientific communication with Sir Humphry Davy after I became a fellow of the Royal Society in 1824, as before that period, and of this I presume Dr. Davy is aware. But if it had been otherwise, I do not see that I could have gone to a fitter source for information than to his printed papers. Whenever I have ventured to follow in the path which Sir Humphry Davy has trod, I have done so with respect and with the highest admiration of his talents, and nothing gave me more pleasure in relation to my last published paper, the Eighth Series, than the thought, that whilst I was helping to elucidate a *still obscure* branch of science, I was able to support the views advanced twenty-eight years ago, and for the first time, by our great philosopher.

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[1] Turner's Elements, Fifth Edit., p. 167.

[2] Quarterly Journal of Science, vol. xxvi. p. 428.

I have such extreme dislike to controversy that I shall not prolong these remarks, and regret much that I have been obliged to make them. I am not conscious of having been unjust to Sir Humphry Davy, to whom I am anxious to give all due honour; but, on the other hand, I feel anxious lest Dr. Davy should inadvertently be doing injury to his brother by attaching a meaning, sometimes of particularity and sometimes of extension, to his words, which I am sure he would never himself have claimed, but which, on the contrary, I feel he has disavowed in saying "that our philosophical systems are very imperfect," and in expressing his confidence "that they *must change more or less with the advancement of science.*" On these points, however, neither Dr. Davy nor myself can now assume to be judges, since with respect to them he has made us both partisans. Dr. Davy has not made me aware of anything that I need change; and I am quite willing to leave the matter as it stands in the printed papers before scientific men, with only this request, which I am sure beforehand will be granted, that such parts of Sir Humphry Davy's papers and my own as relate to the subject in question, be considered both as to their letter and spirit before any conclusion be drawn.

Royal Institution, January 9, 1835.

On the general Magnetic Relations and Characters of the Metals[1].

GENERAL views have long since led me to an opinion, which is probably also entertained by others, though I do not remember to have met ~~with~~ it, that *all* the metals are magnetic in the same manner as iron, though not at common temperatures or under ordinary circumstances[2]. I do not refer to a feeble magnetism[1], uncertain in its existence and source, but to a distinct and decided power, such as that possessed by iron and nickel; and my impression has been that there was a certain temperature for each body, (well known in the case of iron,) beneath which it was magnetic, but above which it lost all power; and that, further, there was some relation between this *point* of temperature, and the *intensity* of magnetic force which the body when reduced beneath it could acquire. In this view iron and nickel were not considered as exceptions from the metals generally with regard to magnetism, any more than mercury could be considered as an exception from this class of bodies as to liquefaction.

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[1] Lond. and Edinb. Phil. Mag., 1836, vol. viii. p. 177.

[2] It may be proper to remark, that the observations made in par. 255 of my "Experimental Researches," have reference only to the three classes of bodies there defined as existing at ordinary temperatures.

I took occasion during the very cold weather of December last, to make some experiments on this point. Pieces of various metals in their pure state were supported at the ends of fine platinum wires, and then cooled to a very low degree by the evaporation of sulphurous acid. They were then brought close to one end of one of the needles of a delicate astatic arrangement, and the magnetic state judged of by the absence or presence of attractive forces. The whole apparatus was in an atmosphere of about 25° Fahr.: the pieces of metal when tried were always far below the freezing-point of mercury, and as judged, generally at from 60° to 70° Fahr. below zero.

The metals tried were,

Arsenic,
Antimony,
Bismuth,
Cadmium,
Cobalt,
Chromium,
Copper,
Gold,
Lead,
Mercury,
Palladium,
Platinum,
Silver,
Tin,
Zinc,

and also Plumbago; but in none of these cases could I obtain the least indication of magnetism.

Cobalt and chromium are said to be both magnetic metals. I cannot find that either of them is so, in its pure state, at any temperatures. When the property was present in specimens supposed to be pure, I have traced it to iron or nickel.

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[1] Encyclop. Metrop. 'Mixed Sciences,' vol. i. p. 761.

The step which we can make downwards in temperature is, however, so small as compared to the changes we can produce in the opposite direction, that negative results of the kind here stated could scarcely be allowed to have much weight in deciding the question under examination, although, unfortunately, they cut off all but two metals from actual comparison. Still, as the only experimental course left open, I proceeded to compare, roughly, iron and nickel with respect to the points of temperature at which they cease to be magnetic. In this respect iron is well known[1]. It loses all magnetic properties at an orange heat, and is then, to a magnet, just like a piece of copper, silver, or any other unmagnetic metal. It does not intercept the magnetic influence between a magnet and a piece of cold iron or a needle. If moved across magnetic curves, a magneto-electric current is produced within it exactly as in other cases. The point at which iron loses and gains its magnetic force appears to be very definite, for the power comes on suddenly and fully in small masses by a small diminution of temperature, and as suddenly disappears upon a small elevation, at that degree.

With nickel I found, as I expected, that the point at which it lost its magnetic relations was very much lower than with iron, but equally defined and distinct. If heated and then cooled, it remained unmagnetic long after it had fallen below a heat visible in the dark: and, in fact, almond oil can bear and communicate that temperature which can render nickel indifferent to a magnet. By a few experiments with the thermometer it appeared that the demagnetizing temperature for nickel is near 630° or 640°. A slight change about this point would either give or take away the full magnetic power of the metal.

Thus the experiments, as far as they go, justify the opinion advanced at the commencement of this paper, that all metals have similar magnetic relations, but that there is a certain temperature for each beneath which it is magnetic in the manner of iron or nickel, and above which it cannot exhibit this property. This magnetic capability, like volatility or fusibility, must depend upon some peculiar relation or condition of the particles of the body; and the striking difference between the necessary temperatures for iron and nickel appears to me to render it far more philosophical to allow that magnetic capability is a general property of all metals, a certain temperature being the essential condition for the development of this state, than to suppose that iron and nickel possess a physical property which is denied to all the other substances of the class.

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[1] See Barlow on the Magnetic Condition of Hot Iron. Phil. Trans., 1822, p. 117, &c.

An opinion has been entertained with regard to iron, that the heat which takes away its magnetic property acts somehow within it and amongst its electrical currents (upon which the magnetism is considered as depending) as flame and heat of a similar intensity act upon conductors charged with ordinary electricity. The difference of temperature necessary for iron and nickel is against this opinion, and the view I take of the whole is still more strongly opposed to it.

The close relation of electric and magnetic phenomena led me to think it probable, that the sudden change of condition with respect to the magnetism of iron and nickel at certain temperatures, might also affect, in some degree, their conducting power for electricity in its ordinary form; but I could not, in such trials as I made, discover this to be the case with iron. At the same time, although sufficiently exact to indicate a great change in conduction, they were not delicate enough to render evident any small change; which yet, if it occurred, might be of great importance in illustrating the peculiarity of magnetic action under these circumstances, and might even elucidate its general nature.

Before concluding this short paper, I may describe a few results of magnetic action, which, though not directly concerned in the argument above, are connected generally with the subject[1]. Wishing to know what relation that temperature which could take from a magnet its power over soft iron, had to that which could take from soft iron or steel its power relative to a magnet, I gradually raised the temperature of a magnet, and found that when scarcely at the boiling-point of almond oil it lost its polarity rather suddenly, and then acted with a magnet as cold soft iron: it required to be raised to a full orange heat before it lost its power as soft iron. Hence the force of the steel to *retain* that condition of its particles which renders it a permanent magnet, gives way to heat at a far lower temperature than that which is necessary to prevent its particles assuming the *same state* by the inductive action of a neighbouring magnet. Hence at one temperature its particles can of themselves retain a permanent state; whilst at a higher temperature, that state, though it can be induced from without, will continue only as long as the inductive action lasts; and at a still higher temperature all capability of assuming this condition is lost.

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[1] See on this subject, Christie on Effects of Temperature, &c. Phil. Trans. 1825, p. 62, &c.

The temperature at which polarity was destroyed appeared to vary with the hardness and condition of the steel.

Fragments of loadstone of very high power were then experimented with. These preserved their polarity at higher temperatures than the steel magnet; the heat of boiling oil was not sufficient to injure it. Just below visible ignition in the dark they lost their polarity, but from that to a temperature a little higher, being very dull ignition, they acted as soft iron would do, and then suddenly lost that power also. Thus the loadstone retained its polarity longer than the steel magnet, but lost its capability of becoming a magnet by induction much sooner. When magnetic polarity was given to it by contact with a magnet, it retained this power up to the same degree of temperature as that at which it held its first and natural magnetism.

A very ingenious magnetizing process, in which electro-magnets and a high temperature are used, has been proposed lately by M. Aimé[1]. I am not acquainted with the actual results of this process, but it would appear probable that the temperature which decides the existence of the polarity, and above which all seems at liberty in the bar, is that required. Hence probably it will be found that a white heat is not more advantageous in the process than a temperature just above or about that of boiling oil; whilst the latter would be much more convenient in practice. The only theoretical reason for commencing at high temperatures would be to include both the hardening and the polarizing degrees in the same process; but it appears doubtful whether these are so connected as to give any advantage in practice, however advantageous it may be to commence the process above the depolarizing temperature.

Royal Institution, Jan. 27, 1836.

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[1] Annales de Chimie et de Physique, tome lvii. p. 442.

Notice of the Magnetic Action of Manganese at Low Temperatures, as stated by M. Berthier[1].

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

THE following fact, stated by M. Berthier, has great interest to me, in consequence of the views I have taken of the general magnetic relations and characters of the metals. As you have done me the favour to publish these views in your Magazine[2], perhaps you will think the present note also worth a place in the next Number.

Berthier, in his *Traité des Essais par la Voie Sèche,* tome i. p. 532, has the following passage in his account of the physical properties of the metals:—"Magnetism.—There are only three metals which are habitually endowed with magnetic force: these are iron, cobalt, and nickel; *but manganese also possesses it beneath a certain degree of temperature much below zero.*" There is no reference to any account of this experimental result, and it is therefore probable that M. Berthier himself has observed the fact, in which case it cannot be doubted; but the result is so important, that any one possessing pure manganese who can verify the result and give an account of the degree of temperature at which the change takes place, will be doing a service to science. The great point will be to secure the *perfect absence* of iron or nickel from the manganese. With respect to cobalt, I have already stated that when pure, I cannot find it to possess magnetic properties at common or low temperatures.

I am, Gentlemen, yours, &c.,

Royal Institution, June 17, 1836. M. FARADAY.

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[1] Lond. and Edinb. Phil. Mag., 1836, vol. ix. p. 65.

[2] See Lond. and Edinb. Phil. Mag., vol. viii. p. 177.—EDIT. or p. 217.

On the general Magnetic Relations and Characters of the Metals: Additional Facts[1].

AN idea that the metals would be all magnetic if made extremely cold, as they are all non-magnetic if above a certain temperature, was put forth in March 1836[2], and some experiments were made, in which several were cooled as low as −60° or −70° Fahr., but without acquiring magnetic powers. It was afterwards noticed[3] that Berthier had said, that besides iron, cobalt, and nickel, ***manganese also possesses magnetic force beneath a certain degree of temperature, much below zero.*** Having had last May the opportunity of working with M. Thilorier's beautiful apparatus for giving both the liquid and the solid state to carbonic acid gas, I was anxious to ascertain what the extremely low temperature procurable by its means would effect with regard to the magnetic powers of metals and other substances, especially with relation to manganese and cobalt; and not having seen any account of similar trials, I send the results to the Philosophical Magazine (if it please the Editors to insert them) as an appendix to the two former notices.

The substances were cooled by immersion in the mixture of ether and solid carbonic acid, and moved either by platina wires attached to them, or by small wooden tongs, also cooled. The temperature, according to Thilorier, would be about 112° below 0° of Fahrenheit. The test of magnetic power was a double astatic needle, each of the two constituent needles being small and powerful, so that the whole system was very sensible to any substance capable of having magnetism induced in it when brought near one of the four poles. Great care was required and was taken to avoid the effect of the downward current of air formed by the cooled body; very thin plates of mica being interposed in the most important cases.

The following metals gave no indications of any magnetic power when thus cooled to −112° Fahr.

Antimony,
Arsenic,
Bismuth,
Cadmium,
Chromium,
Cobalt,

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[1] Lond. and Edinb. Phil. Mag., 1839, vol. xiv. p. 161.

[2] Ibid. vol. viii. p. 177, or p. 217.

[3] Ibid., vol. ix. p. 65, or p. 222.

Copper,
Gold,
Lead,
Mercury,
Palladium,
Platinum,
Rhodium,
Silver,
Tin,
Zinc.

A piece of metallic manganese given to me by Mr. Everett was very slightly magnetic and polar at *common* temperatures. It was not more magnetic when cooled to the lowest degree. Hence I believe the statement with regard to its acquiring such powers under such circumstances to be inaccurate. Upon very careful examination a trace of iron was found in the piece of metal, and to that I think the magnetic property which it possessed must be attributed.

I was very careful in ascertaining that pure *cobalt* did not become magnetic at the very low temperature produced.

The native alloy of iridium and osmium, and also crystals of titanium, were found to be slightly magnetic at common temperatures; I believe because of the presence of iron in them[1]. Being cooled to the lowest degree they did not present any additional magnetic force, and therefore it may be concluded that *iridium, osmium,* and *titanium* may be added as non-magnetic metals to the list already given.

Carbon and the following metallic combinations were then experimented upon in a similar manner, but all the results were negative: not one of the bodies gave the least sign of the acquirement of magnetic power by the cold applied.

1. Carbon.
2. Hæmatite.
3. Protoxide of lead.
4. ———— antimony.
5. ———— bismuth.
6. White arsenic.
7. Native oxide of tin.
8. ———— manganese.
9. Chloride of silver.
10. ———— lead.
11. Iodide of mercury.
12. Galena.
13. Realgar.
14. Orpiment.
15. Dense native cinnabar.
16. Sulphuret of silver.
17. ———— copper.
18. ———— tin.
19. ———— bismuth.
20. ———— antimony.
21. Protosul. iron crystallized.
22. ———— anhydrous.

[[1] See Dr. Wollaston's paper on this subject, Phil. Trans. 1823, Part II., or Phil. Mag. First Series, vol. lxiii. p. 15.—Edit.]

The carbon was the dense hard kind obtained from gas retorts; the substances 3. 4. 5. 6. 9. 10. 11. and some of the sulphurets had been first fused and solidified; and all the bodies were taken in the most solid and dense state which they could acquire.

It is perhaps superfluous to add, except in reference to effects which have been supposed by some to occur in northern latitudes, that the iron and nickel did not appear to suffer any abatement of their peculiar power when cooled to the very lowest degree.

Royal Institution, Feb. 7, 1839.

On a supposed new Sulphuret and Oxide of Antimony[1].

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

IN my Experimental Researches, paragraphs 693. 694. 695. 696., I have, in relation to antimony, described what I considered to be a new sulphuret, and expressed my belief that a new and true protoxide existed consisting of single proportions, "but could not stop to ascertain this matter strictly by analysis." Professor Rose when in London informed me that Berzelius objected to my new sulphuret, and I was induced to make more accurate experiments on that point, which showed me my error, and accorded generally with what Rose had described to me. I intended to publish these results in the first electric paper which I might have to put forth; but my friend Mr. Solly has put into my hands a translation of Berzelius's paper, and it is so clear and accurate as to the facts that I now prefer asking you to publish it, adding merely that my experiments quite agree with those described in it, as regards the sulphuret. With respect to the supposed chloride and oxide, I have not anywhere implied that I had made quantitative experiments on them.

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[1] Lond. and Edinb. Phil. Mag., 1836, vol. viii. p. 476.

On Faraday's supposed Sulphuret of Antimony and Oxide of Antimony; by J. J. BERZELIUS.—*From his* "Jahresbericht," *No.* 15.

"Faraday has stated, that when sulphuret of antimony is heated with more metallic antimony, a new sulphuret of antimony is formed, which when in the fused state is distinguishable from the common sulphuret. According to a few experiments, this sulphuret of antimony is composed of $\mathrm{Sb\,S}$, or one atom of each element. When this sulphuret is dissolved in muriatic acid, sulphuretted hydrogen is evolved, and although a little antimony is separated, yet there remains in solution a combination with chlorine $\mathrm{Sb\,\underline{Cl}}$, which when decomposed with carbonate of soda furnishes a new oxide. The mixing of this with the common oxide is said to have given rise to the contradictory views of its composition, and also to the appearance that the fused oxide of antimony is decomposed to a certain extent by the electric current only until the new oxide is reduced.

"Faraday appears convinced of the truth of this statement, but adds that he has not confirmed by analysis the composition of this oxide, because he should thereby have interrupted the course of his main experiments.

"This appeared to me to deserve a nearer investigation, as well for itself as for the importance of its influence on Faraday's electro-chemical views. I have therefore repeated the above-described experiments of Faraday on the three new combinations of antimony with sulphur, chlorine, and oxygen, and I have found that even if they do exist they cannot possibly be formed by the means which he has described, and they are therefore still to be discovered.

"The following is the substance of my examination. I mixed together very carefully and intimately sulphuret of antimony and metallic antimony in the proportions that, through melting, the combination $\mathrm{Sb}+\mathrm{S}$ must be formed: the mixture was then put into a glass tube: this was drawn out to a capillary end; the air was then expelled by heat, and the tube was hermetically sealed. The tube was then placed in a vessel covered with sand, heated to a full red-heat, and then suffered to cool slowly. When the mass was taken out there was at the bottom a regulus, which contained 63 per cent. of the antimony which had been added after it had been separated from some adhering portions of sulphuret of antimony by boiling with a little muriatic acid.

"This had all the properties of pure antimony. Rubbed to powder and boiled with muriatic acid, it still evolved however a little sulphuretted hydrogen and gave some antimony to the acid. The powder when thus boiled had lost 6¼ per cent.

"From all this it is evident that though the resulting sulphuret of antimony contained more antimony after than before the process, it is not the combination which Faraday thought it was. Even in the cleavage it had not the appearance of a pure sulphuret of antimony. The upper portions had the same radiated structure as the common sulphuret of antimony, and a few larger crystals had shot up into the upper surface of the regulus, where they were surrounded with an irregular mass of a lighter colour. The upper and the lower portions of this so-formed antimony were each separately analysed, in such a manner that a weighed portion was put into muriatic acid and digested in it in the water-bath. The solution went on rapidly. From the lowermost portion crystals fell off one after another, upon which the acid did not act. The same happened likewise with the uppermost portion, only they were smaller and fewer in number. These insoluble parts when well boiled and washed were from the lowermost 15 and from the uppermost 10 per cent. It proved to be pure metallic antimony formed in feathery crystals, and shows, therefore, the interesting fact that sulphuret of antimony can dissolve at a high temperature 13¼ per cent. of metallic antimony, which when the solution is suffered to cool sufficiently slowly crystallizes out of the yet fluid sulphuret of antimony before this latter solidifies. By a more rapid cooling the whole mass congeals together, and the cleavage is then quite similar throughout.

"From what has been said it is quite evident that the muriatic acid takes up nothing but the common chloride of antimony. I have examined this behaviour further in detail, and thereby found, that by this method neither with water nor alkali is it possible to obtain any other oxide.

"The above-mentioned experiment of Faraday, that melted oxide of antimony is decomposed by the electric current, clearly proves that the law proposed by him, that similar quantities of electricity always evolve equal chemical proportions, only holds good so long as the comparison is made between combinations of proportional composition.

"As for the cause of the appearance, that the decomposition of the oxide of antimony becomes gradually weaker and weaker, and at last ceases, it is evident that Faraday has overlooked the circumstance that the oxide is decomposed into metal at the negative conductor and antimonious acid at the positive conductor, which then soon becomes encrusted with a solid substance, after which the electricity could not have any further action."

With respect to Berzelius's objection in the last paragraph but one of his paper, I will ask you to reprint paragraph 821. of my series. "All these facts combine into, I think, an irresistible mass of evidence, proving the truth of the important proposition which I at first laid down, namely, *that the chemical power of a current of electricity is in direct proportion to the absolute quantity of electricity which passes.* (377. 783.) They prove too that this is not merely true with one substance, as water, but generally with all electrolytic bodies; and further that the results obtained with any *one substance* do not merely agree amongst themselves, but also with those obtained from *other substances,* the whole combining together into *one series of definite electro-chemical actions.* (505.) I do not mean to say that no exceptions will appear; perhaps some may arise, especially amongst substances existing only by weak affinity: but I do not expect that any will seriously disturb the result announced. If, in the well-considered, well-examined, and I may surely say, well-ascertained doctrines of the definite nature of ordinary chemical affinity, such exceptions occur, as they do in abundance, yet without being allowed to disturb our minds as to the general conclusion, they ought also to be allowed, if they should present themselves at this the opening of a new view of electro-chemical action; not being held up as obstructions to those who may be engaged in rendering that view more and more perfect, but laid aside for a while, in hopes that their perfect and consistent explanation will finally appear."

With regard to my having overlooked the cause of the diminution and cessation of voltaic action on the oxide of antimony, I do not know how that can well be said, for Berzelius's statement seems in parts to be almost a copy of the reasons I have given: see paragraph 801. of the Seventh Series of my Researches. My explanation is actually referred to in the account of the action on the oxide of antimony at paragraph 693., but by a misprint 802. has been stated instead of 801[1].

I am, Gentlemen, yours, &c.

M. FARADAY.

On the History of the Condensation of the Gases, in reply to Dr. Davy, *introduced by some Remarks on that of Electro-magnetic Rotation*[2].

MY DEAR SIR, Royal Institution, May 10, 1836.

I HAVE just concluded looking over Dr. Davy's Life of his brother Sir Humphry Davy. In it, between pages 160 and 164 of the second volume, the author links together some account, with observations, of the discovery of electro-magnetic rotation, and that of the condensation of the gases, concluding at page 164 with these words: "I am surprised that Mr. Faraday has not come forward to do him [Sir Humphry Davy] justice. As I view the matter, it appears hardly less necessary to his own honest fame than his acknowledgement to Dr. Wollaston, on the subject of the first idea of the rotary magnetic motion."

I regret that Dr. Davy by saying this has made that necessary which I did not before think so; but I feel that I cannot after his observation indulge my earnest desire to be silent on the matter without incurring the risk of being charged with something opposed to an *honest* character. This I dare not risk; but in answering for myself, I trust it will be understood that I have been driven unwillingly into utterance.

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[1] This reference is correctly made in Lond. and Edinb. Phil. Mag., vol. v. p. 170.—EDIT.

[2] Lond. and Edinb. Phil. Mag. 1836, vol. viii. p. 521.

Dr. Davy speaks of electro-magnetic rotation, and so also must I, for the purpose of showing certain coincidences in dates, &c. between the latter part of that affair and the condensation of chlorine and the gases, &c. Œrsted's experiments were published in Thomson's Annals of Philosophy for October 1820, and from this, I believe, was derived the first knowledge of them which we had in this country. At all events it was the first intimation Sir Humphry Davy and I had of them, for he brought down the Number into the laboratory on the morning of its appearance (October 1st) and we repeated the experiments together. I may remark that this is a proof that Dr. Davy, in the Life[1] as well as elsewhere[2], does not always understand the meaning of his brother's words, and I think that he would never have written the lines which have driven me to the present and a former reply[3] if he had.

Immediately upon Œrsted's great discovery, the subject was pursued earnestly, and various papers were written, amongst which is one by Sir Humphry Davy, Phil. Trans. 1821, page 7, read before the Royal Society November 16, 1820, in which, at page 17, he describes the rolling of certain wires upon knife-edges, being *attracted* when the north pole of the magnet was presented under certain conditions of current, and *repelled* under certain other conditions of current, &c.

Another paper was a brief statement by the Editor of the Quarterly Journal of Science, (Mr. Brande,) in which he announces distinctly and clearly Dr. Wollaston's view of the nature of the electro-magnetic force, and its circumferential character. It is in the tenth volume, p. 363, and may be dated according to the number of the Journal, 1st January 1821.

Then there are my historical sketches in the Annals of Philosophy, N.S., vols. ii. and iii. written in July, August, and September 1821, and the paper describing my discovery of the electro-magnetic rotation dated 11th September 1821[4], and others; but we will pass on to that of Sir Humphry Davy, read 6th March 1823[1], which with its consequents is *synchronous* with the affair of the condensation of gases. This is the paper which Dr. Davy says "he (Sir H. D.,) concludes by an act of justice to Dr. Wollaston, pointing out how the discovery of the *rotations of the electro-magnetic wire round its axis* by the approach of a magnet, realized by the ingenuity of Mr. Faraday, had been anticipated, and even attempted by Dr. Wollaston in the laboratory of the Royal Institution[2]."

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[1] Vol. ii. p. 143.

[2] Lond. and Edinb. Phil. Mag., 1835, vol. vii. p. 340; or p. 215 of this volume.

[3] Ibid. p. 337; or page 211 of this volume.

[4] Quarterly Journal of Science, vol. xii. p. 74; or page 127 of this volume.

I have elsewhere[3] done full justice to Dr. Wollaston on the point of electro-magnetic rotation, and have no desire to lessen the force of anything I have said, but would rather exalt it. But as Dr. Davy has connected it with the condensation of the gases, I must show the continual tendency to error which has occurred in both these matters. Dr. Davy, then, is in error when he says I realized Dr. Wollaston's expectation; nor does Sir Humphry Davy say what his brother imputes to him. *I did not realize* the rotations of the electro-magnetic wire round its axis; that fact was discovered by M. Ampère, at a later date; and even after I had discovered the rotation of the wire round the magnet as a centre, and that of the magnet round the wire, I could not succeed in causing the wire to revolve on its own axis[4]. The result which Wollaston very philosophically and beautifully deduced from his principles, and which he tried to obtain in the laboratory, was, that wires could be caused to roll, not by attraction and repulsion as had been effected by Davy[5], but by a tangential action, according to the principles which had been already made known to the public as his (Dr. W.'s) by Mr. Brande[6].

What Sir Humphry Davy says in his printed paper[7] is this: "I cannot with propriety conclude without mentioning a circumstance in the history of the progress of electro-magnetism, which, though well known to many Fellows of this Society, has, I believe, never been made public, namely, that we owe to the sagacity of Dr. Wollaston the first idea of the possibility of *the rotations of the electro-magnetic wire round its axis by the approach of a magnet*; and I witnessed early in 1821 an unsuccessful experiment which he made to produce the effect in the laboratory of the Royal Institution." This paper being read on the 6th of March 1823, was reported on the first of the following month in the Annals of Philosophy, N.S., vol. v. p. 304; the reporter giving altogether a different sense to what is conveyed by Sir Humphry Davy's printed paper, and saying that "had not an experiment on the subject made by Dr. W. in the laboratory of the Royal Institution, and witnessed by Sir Humphry, failed, *merely through an accident* which happened to the apparatus, *he would have been the discoverer of that phenomenon*[1]."

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[1] Phil. Trans. 1823, p. 153.

[2] Life, vol. ii. p. 160.

[3] Quarterly Journal of Science, vol. xv. p. 288; or page 159 of this volume.

[4] Ibid., vol. xii. p. 79; or page 131 of this volume.

[5] Phil. Trans. 1821, p. 17.

[6] Quarterly Journal of Science, vol. x. p. 363.

[7] Phil. Trans. 1823, p. 158.

I have an impression that this report of the paper was first made known to me by Sir Humphry Davy himself, but a friend's recollection makes me doubtful on this point: however, Sir Humphry, when first he adverted to the subject, told me it was inaccurate and very unjust; and advised me to draw up a contradiction which the Editor should insert the next month. I drew up a short note, and submitting it to Sir Humphry he altered it and made it what it appears in the May Number of the Annals of Philosophy, N.S. vol. v. p. 391, as from the Editor, all the parts from "but writing only" to the end being Sir Humphry's; and I have the manuscript in *his hand-writing* inserted as an illustration into my copy of Paris's Life of Davy.

The whole paragraph stands thus: "⁂ We endeavoured last month to give a full report of the important paper communicated by the President to the Royal Society on the 5th [6th] of March[2]; but writing only from memory, we have made two errors, one with respect to the rotation of the mercury not being stopped, but produced, by the approximation of the magnet; the other in the historical paragraph in the conclusion, which, as we have stated it, is unjust to Mr. Faraday, and does not at all convey the sense of the author. We wish, therefore, to refer our readers forward to the original paper, when it shall be published, for the correction of these mistakes.—*Edit.*"

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[1] In justice to the reporter, I have sought carefully at the Royal Society for the original manuscript, being the paper which he heard read; but it cannot be found in its place.

[2] So far is mine; the rest is Sir Humphry Davy's.

From this collection of dates and documents any one may judge that I at all events was *unjustly* subject to some degree of annoyance, and they will be the more alive to this if they recollect that all these things were happening at the very time of the occurrence of the condensation of gases and its consequences, and during the time that my name was before the Royal Society as a candidate for its fellowship. I do not believe that any one was wittingly the cause of this state of things, but all seemed confusion, and generally to my disadvantage. For instance, this very paper of Sir Humphry Davy's which contains the "act of justice," as Dr. Davy calls it, is entitled, "*On a new phenomenon of Electro-magnetism.*" Yet what is electro-magnetic was not new, but merely another form of my rotation; and the *new phenomenon* is purely electrical, being the same as that previously discovered by M. Ampère. As M. Ampère's result is described for the first time in a paper of the date of the 4th of September 1822[1], and Sir Humphry Davy's paper was read as soon after as the 6th of March 1823[2], the latter probably did not know of the result which the former had obtained.

To conclude this matter: in consequence of these and other circumstances, and the simultaneous ones respecting the condensation of chlorine, I wrote the historical statement[3], to which Dr. Davy refers[4], in which, admitting everything that Dr. Wollaston had done, I claim and *prove* my right to the discovery of the rotations I had previously described. This paper before its publication I read with Dr. Wollaston; he examined the proofs which I have adduced at p. 291 (page 161 of this volume), and after he had made a few alterations he brought it into the state in which it is printed, expressed his satisfaction at the arguments and his approval of the whole. The copy I have preserved, and I will now insert the most considerable and important of Dr. Wollaston's corrections as an illustration. At the end of the paragraph at the bottom of page 291 (page 162 of this volume), I had expressed the sense thus: "But what I thought to be attraction and repulsion in August 1821, Dr.

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[1] Ann. de Chim., 1822, vol. xxi. p. 47.

[2] Phil. Trans. 1823, p. 153.

[3] Quarterly Journal of Science, vol. xv. p. 288; or page 159 of this volume.

[4] Life, vol. ii. p. 146, bottom of the page.

Wollaston long before perceived to be an impulsion in one direction only, and upon that knowledge founded his expectations." This he altered to: "But what I thought to be attraction *to* and repulsion *from the wire* in August 1821, Dr. Wollaston long before perceived to arise from *a power not directed to or from the wire, but acting circumferentially round it as axis,* and upon that knowledge founded his expectation." The parts in Italics are in his hand-writing.

[The remainder of this letter regards the condensation of the gases, which as it has no connexion with electricity or magnetism, I omit, with the exception of the concluding paragraph, which is as follows.]

Believing that I have now said enough to preserve my own "honest fame" from any injury it might have risked from the mistakes of Dr. Davy, I willingly bring this letter to a close, and trust that I shall never again have to address you on the subject.

I am, my dear Sir, yours, &c.

Richard Phillips, Esq., &c. &c. M. FARADAY.

On a peculiar Voltaic Condition of Iron, by Professor SCHOENBEIN, *of Bâle*; *in a Letter to* Mr. Faraday: *with further Experiments on the same subject, by* Mr. FARADAY, *communicated in a Letter to* Mr. Phillips[1].

To Michael Faraday, D.C.L., F.R.S., &c.

SIR,

As our continental and particularly German periodicals are rather slow in publishing scientific papers, and as I am anxious to make you as soon as possible acquainted with some new electro-chemical phenomena lately observed by me, I take the liberty to state them to you by writing. Being tempted to do so only by scientific motives, I entertain the flattering hope that the contents of my letter will be received by you with kindness. The facts I am about laying before you seem to me not only to be new, but at the same time deserving the attention of chemical philosophers. *Les voici.*

If one of the ends of an iron wire be made red hot, and after cooling be immersed in nitric acid, sp. gr. 1·35, neither the end in question nor any other part of the wire will be affected, whilst the acid of the said strength is well known to act rather violently upon common iron. To see how far the influence of the oxidized end of the wire goes, I took an iron wire of 50′ in length and 0‴·5 in thickness, heated one of its ends about 3″ in length, immersed it in the acid of the strength above mentioned, and afterwards put the other end into the same fluid. No action of the acid upon the iron took place. From a similar experiment made upon a cylindrical iron bar of 16′ in length and 4‴ diameter the same result was obtained. The limits of this protecting influence of oxide of iron with regard to quantities I have not yet ascertained; but as to the influence of heat, I found that above the temperature of about 75° the acid acts in the common way upon iron, and in the same manner also, at common temperatures, when the said acid contains water beyond a certain quantity, for instance, 1, 10, 100, and even 1000 times its volume. By immersing an iron wire in nitric acid of sp. gr. 1·5 it becomes likewise indifferent to the same acid of 1·35.

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[1] Lond. and Edinb. Phil. Mag., 1836, vol. ix. p. 53.

But by far the most curious fact observed by me is, that any number of iron wires may be made indifferent to nitric acid by the following means. An iron wire with one of its ends oxidized is made to touch another common iron wire; both are then introduced into nitric acid of sp. gr. 1·35, so as to immerse the oxidized end of the one wire first into the fluid, and have part of both wires above the level of the acid. Under these circumstances no chemical action upon the wires will take place, for the second wire is, of course, but a continuation of that provided with an oxidized end. But no action occurs, even after the wires have been separated from each other. If the second wire having become indifferent be now taken out of the acid and made to touch at any of its parts not having been immersed a third wire, and both again introduced into the acid so as to make that part of the second wire which had previously been in the fluid enter first, neither of the wires will be acted upon either during their contact or after their separation. In this manner the third wire can make indifferent or passive a fourth one, and so on.

Another fact, which has as yet, as far as I know, not been observed, is the following one. A wire made indifferent by any of the means before mentioned is immersed in nitric acid of sp. gr. 1·35, so as to have a considerable part of it remaining out of the fluid; another common wire is put into the same acid, likewise having one of its ends rising above the level of the fluid. The part immersed of this wire will, of course, be acted upon in a lively manner. If the ends of the wires which are out of the acid be now made to touch one another, the indifferent wire will instantly be turned into an active one, whatever may be the lengths of the parts of the wires not immersed. [If there is any instance of chemical affinity being transmitted in the form of a current by means of conducting bodies, I think the fact just stated may be considered as such.] It is a matter of course that direct contact between the two wires in question is not an indispensably necessary condition for communicating chemical activity from the active wire to the passive one; for any metal connecting the two ends of the wires renders the same service.

Before passing to another subject, I must mention a fact, which seems to be one of some importance. An iron wire curved into a fork is made to touch at its bend, a wire provided with an oxidized end; in this state of contact both are introduced into nitric acid of sp. gr. 1·35 and 30°, so as first to immerse in the acid the oxidized end; the fork will, of course, not be affected. If now a common iron wire be put into the acid, and one of the ends of the fork touched by it, this end will immediately be acted upon, whilst the other end remains passive; but as soon as the iron wire with the oxidized end is put out of contact with the bend of the fork, its second end is also turned active. If the parts of the fork rising above the level of the acid be touched by an iron wire, part of which is immersed and active in the acid, no communication of chemical activity will take place, and both ends of the fork remain passive; but by the removal of the iron wire (with the oxidized end) from the bend of the fork this will be thrown into chemical action.

As all the phenomena spoken of in the preceding lines are, no doubt, in some way or other dependent upon a peculiar electrical state of the wires, I was very curious to see in what manner iron would be acted upon by nitric acid when used as an electrode. For this purpose I made use of that form of the pile called the *couronne des tasses,* consisting of fifteen pairs of zinc and copper. A platina wire was connected with (what we call) the negative pole of the pile, an iron wire with the positive one. The free end of the platina wire was first plunged into nitric acid sp. gr. 1·35, and by the free end of the iron wire the circuit closed. Under these circumstances the iron was not in the least affected by the acid; and it remained indifferent to the fluid not only as long as the current was passing through it, but even after it had ceased to perform the function of the positive electrode. The iron wire proved, in fact, to be possessed of all the properties of what we have called a passive one. If such a wire is made to touch the negative electrode, it instantaneously becomes an active one, and a nitrate of iron is formed; whether it be separate from the positive pole or still connected with it, and the acid be strong or weak.

But another phenomenon is dependent upon the passive state of the iron, which phenomenon is in direct contradiction with all the assertions hitherto made by philosophical experimenters. The oxygen at the anode arising from the decomposition of water contained in the acid, does not combine with the iron serving as the electrode, but is evolved at it, just in the same manner as if it were platina, and to such a volume as to bear the ratio of 1 : 2 to the quantity of hydrogen evolved at the cathode. To obtain this result I made use of an acid containing 20 times its volume of water; I found, however, that an acid containing 400 times its volume of water still shows the phenomenon in a very obvious manner. But I must repeat it, the indispensable condition for causing the evolution of the oxygen at the iron wire is to close the circuit exactly in the same manner as above mentioned. For if, *exempli gratiâ,* the circuit be closed with the negative platina wire, not one single bubble of oxygen gas makes its appearance at the positive iron; neither is oxygen given out at it, when the circuit is closed, by plunging first one end of the iron wire into the nitric acid, and by afterwards putting its other end in connexion with the positive pole of the pile. In both cases a nitrate of iron is formed, even in an acid containing 400 times its volume of water; which salt may be easily observed descending from the iron wire in the shape of brownish-yellow-coloured streaks.

I have still to state the remarkable fact, that if the evolution of oxygen at the anode be ever so rapidly going on, and the iron wire made to touch the negative electrode within the acid, the disengagement of oxygen is discontinued, not only during the time of contact of the wires, but after the electrodes have been separated from each other. A few moments holding the iron wire out of the acid is, however, sufficient to recommunicate to it the property of letting oxygen gas evolve at its surface. By the same method the wire acquires its evolving power again, whatever may have been the cause of its loss. The evolution of oxygen also takes place in dilute sulphuric and phosphoric acids, provided, however, the circuit be closed in the manner above described. It is worthy of remark, that the disengagement of oxygen at the iron in the last-named acids is much easier stopt, and much more difficult to be caused again, than is the case in nitric acid. In an aqueous solution of caustic potash, oxygen is evolved at the positive iron, in whatever manner the circuit may be closed; but no such disengagement takes place in aqueous solutions of hydracids, chlorides, bromides, iodides, fluorides. The oxygen, resulting in these cases from the decomposition of water, and the anion (chlorine, bromine, &c.) of the other electrolyte decomposed combine at the same time with the iron.

To generalize these facts, it may be said, that independently of the manner of closing the circuit, oxygen is always disengaged at the positive iron, provided the aqueous fluid in which it is immersed do not (in a sensible manner) chemically act upon it; and that no evolution of oxygen at the anode in contact with iron under any circumstances takes place, if besides oxygen another anion is set free possessed of a strong affinity for iron. This metal having once had oxygen evolved at itself, proves always to be indifferent to nitric acid of a certain strength, whatever may be the chemical nature of the fluid in which the phenomenon has taken place.

I have made a series of experiments upon silver, copper, tin, lead, cadmium, bismuth, zinc, mercury, but none showed any resemblance to iron, for all of them were oxidized when serving as positive electrodes. Having at this present moment neither cobalt nor nickel at my command, I could not try these magnetic metals, which I strongly suspect to act in the same manner as iron does.

It appears from what I have just stated that the anomalous bearing of the iron has nothing to do with its degree of affinity for oxygen, but must be founded upon something else. Your sagacity, which has already penetrated into so many mysteries of nature, will easily put away the veil which as yet covers the phenomenon stated in my letter, in case you should think it worth while to make it the object of your researches.

Before I finish I must beg of you the favour of overlooking with indulgence the many faults I have, no doubt, committed in my letter. Formerly I was tolerably well acquainted with your native tongue; but now, having been out of practice in writing or speaking it, it is rather hard work to me to express myself in English.

It is hardly necessary to say that you may privately or publicly make any use of the contents of this letter.

I am, Sir, your most obedient Servant,

C. T. SCHOENBEIN,

Bâle, May 17, 1836. Prof. of Chem. in the University of Bâle.

DEAR PHILLIPS,

The preceding letter from Professor Schoenbein, which I received a week or two ago, contains facts of such interest in relation to the first principles of chemical electricity, that I think you will be glad to publish it in your Philosophical Magazine. I send it to you unaltered, except in a word or two here and there; but am encouraged by what I consider the Professor's permission (or rather the request with which he has honoured me), to add a few results in confirmation of the effects described, and illustrative of some conclusions that may be drawn from the facts.

The influence of the oxidized iron wire, the transference of the inactive state from wire to wire, and the destruction of that state, are the facts I have principally verified; but they are so well described by Professor Schoenbein that I will not add a word to what he has said on these points, but go at once to other results.

Iron wire, as M. Schoenbein has stated, when put *alone* into strong nitric acid, either wholly or partly immersed, acquires the peculiar inactive state. This I find takes place best in a long narrow close vessel, such as a tube, rather than in a flat broad open one like a dish. When thus rendered quiescent by itself, it has the same properties and relations as that to which the power has been communicated from other wires.

If a piece of ordinary iron wire be plunged wholly or in part into nitric acid of about specific gravity 1·3 or 1·35, and after action has commenced it be touched by a piece of platina wire, also dipping into the acid, the action between the acid and the iron wire is instantly stopped. The immersed portion of the iron becomes quite bright, and *remains* so, and is in fact in the same state, and can be used in the same manner as the iron rendered inactive by the means already described. This protecting power of platina with respect to iron is very constant and distinct, and is the more striking as being an effect the very reverse of that which might have been anticipated prior to the knowledge of M. Schoenbein's results. It is equally exerted if the communication between it and the iron is not immediate, but made by other metals; as, for instance, the wire of a galvanometer; and if circumstances be favourable, a small surface of platina will reduce and nullify the action of the acid upon a large surface of iron.

This effect is the more striking if it be contrasted with that produced by zinc; for the latter metal, instead of protecting the iron, throws it into violent action with the nitric acid, and determines its quick and complete solution. The phenomena are well observed by putting the iron wire into nitric acid of the given strength, and touching it in the acid alternately by pieces of platina and zinc: it becomes active or inactive accordingly; being preserved by association with the platina, and corroded by association with the zinc. So also, as M. Schoenbein has stated, if iron be made the negative electrode of a battery containing from two to ten or more pairs of plates in such acid, it is violently acted upon; but when rendered the positive electrode, although oxidized and dissolved, the process, comparatively, is extremely slow.

Gold has the same power over iron immersed in the nitric acid that platina has. Even silver has a similar action; but from its relation to the acid, the effect is attended with peculiar and changeable results, which I will refer to hereafter.

A piece of box-wood charcoal, and also charcoal from other sources, has this power of preserving iron, and bringing it into the inactive state. Plumbago, as might be expected, has the same power.

When a piece of bright steel was first connected with a piece of platina, then the platina dipped into the acid, and lastly the steel immersed, according to the order directed in the former cases by Professor Schoenbein, the steel was preserved by the platina, and remained clear and bright in the acid, even after the platina was separated from it, having, in fact, the properties of the inactive iron. When immersed of itself, there was at first action of the usual kind, which, being followed by the appearance of the black carbonaceous crust, known so well in the common process of examining steel, the action immediately ceased, and the steel was preserved, not only at the part immersed, but upon introducing a further portion, it also remained clean and bright, being actually protected by association with the carbon evolved on the part first immersed.

When the iron is in this peculiar inactive state, as M. Schoenbein has stated, there is not the least action between it and the nitric acid. I have retained such iron in nitric acid, both alone and in association with platina wire for 30 days, without change; the metal has remained perfectly bright, and not a particle has been dissolved.

A piece of iron wire in connexion with platina wire was entirely immersed in nitric acid of the given strength, and the latter gradually heated. No change took place until the acid was nearly at the boiling-point, when it and the iron suddenly entered into action, and the latter was instantly dissolved.

As an illustration of the extent and influence of this state, I may mention, that with a little management it can be shown that the iron has lost, when in the peculiar state, even its power of precipitating copper and other metals. A mixture of about equal parts of a solution of nitrate of copper and nitric acid was made. Iron in the ordinary, or even in the peculiar state, when put into this solution, acted, and copper was precipitated; but if the inactive iron was first connected with a piece of platina dipping into the solution, and then its own prepared surface immersed, after a few seconds the platina might be removed, and the iron would remain pure and bright for some time. At last it usually started into activity, and began to precipitate copper, being itself rapidly corroded. When silver is the metal in solution, the effect is still more striking, and will be referred to immediately.

I then used a galvanometer as the means of connexion between the iron and other metals thus associated together in nitric acid, for the purpose of ascertaining, by the electric currents produced, in what relative condition the metals stood to each other; and I will, in the few results I may have to describe, use the relations of platina and zinc to each other as the terms of comparison by which to indicate the states of these metals under various circumstances.

The oxidized iron wire of Professor Schoenbein is, when in association with platina, exactly as another piece of platina would be. There is no chemical action, nor any electric current. The iron wire, rendered inactive either by association with the oxidized wire or in any other way, is also as platina to the platina, and produces no current.

When ordinary iron and platina in connexion by means of the galvanometer are dipped into the acid, (it matters not which first,) there is action at the first moment on the iron, and a very strong electric current, the iron being as zinc to the platina. The action on the iron is, however, soon stopped by the influence of the platina, and then the current instantly ceases, the iron now acting as platina to the platina. If the iron be lifted into the air for a moment until action recommences on it, and be then reimmersed, it again produces a current, acting as zinc to the platina; but as before, the moment the action stops, the current is stopt also.

If an active or ordinary, and an inactive or peculiar iron wire be both immersed in the nitric acid separately, and then connected either directly or through the galvanometer, the second does not render the first inactive, but is itself thrown into action by it. At the first moment of contact, however, a strong electric current is formed, the first iron acting as zinc, and the second as platina. Immediately that the chemical action is re-established at the second as well as the first, all current ceases, and both pieces act like zinc. On touching either of them in the acid with a piece of platina, both are protected, and cease to act; but there is no current through the galvanometer, for both change together.

When iron was associated with gold or charcoal, the phenomena were the same. Using steel instead of iron, like effects ensued.

One of the most valuable results in the present state of this branch of science which these experiments afford, is the additional proof that *voltaic electricity is due to chemical action, and not to contact.* The proof is equally striking and decisive with that which I was able to give in the Eighth Series of my Experimental Researches (par. 880). What indeed can show more evidently that the current of electricity is due to chemical action rather than to contact, than the fact, that though the contact is continued, yet when the chemical action ceases, the current ceases also?

It might at first be supposed, that in consequence of the peculiar state of the iron, there was some obstacle, not merely to the *formation* of a current, but to the *passage* of one; and that, therefore, the current which metallic contact tended to produce could not circulate in the system. This supposition was, however, negatived by removing the platina wire into a second cup of nitric acid, and then connecting the two cups by a compound platina and iron wire, putting the platina into the first vessel, and the iron attached to it into the second. The second wire acted at the first moment, producing its corresponding current, which passed through the first cup, and consequently through the first and inactive wire, and affected the galvanometer in the usual way. As soon as the second iron was brought into the *peculiar* condition, the current of course ceased; but that very cessation showed that the electric current was not stopped by a want of conducting power, or a want of metallic contact, for both remained unchanged, but by the absence of chemical action. These experiments, in which the current ceases whilst contact is continued, combined with those I formerly gave, in which the current is produced though contact does not exist, form together a perfect body of evidence in respect to this elementary principle of voltaic action.

With respect to the state of the iron when inactive in the nitric acid, it must not be confounded with the inactive state of amalgamated or pure zinc in dilute sulphuric acid. The distinction is easily made by the contact of platina with either in the respective acids, for with the iron such association does nothing, whereas with the zinc it developes the full force of that metal and generates a powerful electric current. The iron is in fact as if it had no attraction for oxygen, and therefore could not act on the electrolyte present, and consequently could produce no current. My strong impression is that the surface of the iron is oxidized, or that the superficial particles of the metal are in such relation to the oxygen of the electrolyte as to be equivalent to an oxidation; and that having thus their affinity for oxygen satisfied, and not being dissolved by the acid under the circumstances, there is no renewal of the metallic surface, no reiteration of the attraction of successive particles of the iron on the elements of successive portions of the electrolyte, and therefore not those successive chemical actions by which the electric current (which is definite in its production as well as in its action) can be continued.

In support of this view, I may observe, that in the first experiment described by Professor Schoenbein, it cannot be doubted that the formation of a coat of oxide over the iron when heated is the cause of its peculiar and inactive state: the coat of oxide is visible by its colour. In the next place, all the forms of experiment by which this iron, or platina, or charcoal, or other voltaic arrangements are used to bring ordinary iron into the peculiar state, are accompanied by a determination of oxygen to the surface of the iron; this is shown by the electric current produced at the first moment, and which in such cases always precedes the change of the iron from the common to the peculiar state. That the coat of oxide produced by common means might be so thin as not to be sensible and yet be effectual, was shown by heating a piece of iron an inch or two from the end, so that though blue at the heated part, the end did not seem in the slightest degree affected, and yet that end was in the peculiar state. Again, whether the iron be oxidized in the flame much or only to the very slight degree just described, or be brought into the peculiar state by voltaic association with other pieces or with platina, &c., still if a part of its surface were removed even in the smallest degree and then the new surface put into contact with the nitric acid, that part was at the first moment as common iron; the state being abundantly evident by the electrical current produced at the instant of immersion.

Why the superficial film of oxide, which I suppose to be formed when the iron is brought into the peculiar state by voltaic association, or occasionally by immersion alone into nitric acid, is not dissolved by the acid, is I presume dependent upon the peculiarities of this oxide and of nitric acid of the strength required for these experiments; but as a matter of fact it is well known that the oxide produced upon the surface of iron by heat, and showing itself by thin films of various colours, is scarcely touched by nitric acid of the given strength though left in contact with it for days together. That this does not depend upon the film having any great thickness, but upon its peculiar condition, is rendered probable from the fact, that iron oxidized by heat, only in that slight degree as to offer no difference to the eye, has been left in nitric acid of the given strength for weeks together without any change. And that this mode of superficial oxidation, or this kind of oxide, may occur in the voltaic cases, is rendered probable by the results of the oxidation of iron in nitrate of silver. When nitrate of silver is fused and common iron dipped into it, so as to be thoroughly wetted, being either alone or in association with platina, the iron does not commence a violent action on the nitrate and throw down silver, but it is gradually oxidized on the surface with exactly the same appearances of colour, uniformity of surface, &c., as if it were slowly oxidized by heat in the air.

Professor Schoenbein has stated the case of iron when acting as the positive electrode of a *couronne des tasses*. If that instrument be in strong action, or if an ordinary battery be used containing from two to ten or more plates, the positive iron instantly becomes covered in the nitric acid with a coat of oxide, which though it does not adhere closely still is not readily dissolved by the acid when the connexion with the battery is broken, but remains for many hours on the iron, which itself is in the peculiar inactive state. If the power of the voltaic apparatus be very weak, the coat of oxide on the iron in the nitric acid often assumes a blue tint like that of the oxide formed by heat. A part of the iron is however always dissolved in these cases.

If it be allowed that the surface particles of the iron are associated with oxygen, are in fact oxidized, then all the other actions of it in combination with common iron and other metals will be consistent; and the cause of its platina-like action, of its forming a strong voltaic current with common iron in the first instance, and then being thrown into action by it, will be explained by considering it as having the power of determining and disposing of a certain portion of hydrogen from the electrolyte at the first moment and being at the same time brought into a free metallic condition on the surface so as to act afterwards as ordinary iron.

I need scarcely refer here to the probable existence of a very close connexion between the phenomena which Professor Schoenbein has thus pointed out with regard to iron, and those which have been observed by others, as Ritter and Marianini, with regard to secondary piles, and A. De la Rive with respect to peculiar affections of platina surfaces.

In my Experimental Researches (par. 476.) I have recorded a case of voltaic excitement, which very much surprised me at the time, but which I can now explain. I refer to the fact stated, that when platina and iron wire were connected voltaically in association with fused nitrate or chloride of silver, there was an electric current produced, but in the reverse direction to that expected. On repeating the experiment, I found that when iron was associated with platina or silver in fused nitrate or chloride of silver, there was occasionally no current, and when a current did occur it was almost constantly as if the iron was as platina, the silver or platina used being as zinc. In all such cases, however, it was a thermo-electric current which existed. The volta-electric current could not be obtained, or lasted only for a moment.

When iron in the peculiar inactive state was associated with silver in nitric acid sp. gr. 1·35, there was an electric current, the iron acting as platina; the silver gradually became tarnished and the current continued for some time. When ordinary iron and silver were used in the nitric acid there was immediate action and a current, the iron being as zinc, to the silver as platina. In a few moments the current was reversed, and the relation of the metals was also reversed, the iron being as platina, to the silver as zinc; then another inversion took place, and then another, and thus the changes went on sometimes eight or nine times together, ending at last generally in a current constant in its direction, the iron being as zinc, to the silver as platina: occasionally the reverse was the case, the predominant current being as if the silver acted as zinc.

This relation of iron to silver, which was before referred to page 242, produces some curious results as to the precipitation of one metal by another. If a piece of clean iron is put into an aqueous solution of nitrate of silver, there is no immediate apparent change of any kind. After several days the iron will become slightly discoloured, and small irregular crystals of silver will appear; but the action is so slow as to require time and care for its observation. When a solution of nitrate of silver to which a little nitric acid had been added was used, there was still no sensible immediate action on the iron. When the solution was rendered very acid, then there was direct immediate action on the iron; it became covered with a coat of precipitated silver: the action then suddenly ceased, the silver was immediately redissolved, and the iron left perfectly clear, in the peculiar condition, and unable to cause any further precipitation of the silver from the solution. It is a remarkable thing in this experiment to see the silver rapidly dissolve away in a solution which cannot touch the iron, and to see the iron in a clean metallic state unable to precipitate the silver.

Iron and platina in an aqueous solution of nitrate of silver produce no electric current; both act as platina. When the solution is rendered a little acid by nitric acid, there is a very feeble current for a moment, the iron being as zinc. When still more acid is added so as to cause the iron to precipitate silver, there is a strong current whilst that action lasts, but when it ceases the current ceases, and then it is that the silver is redissolved. The association of the platina with the iron evidently helps much to stop the action.

When iron is associated with mercury, copper, lead, tin, zinc, and some other metals, in an aqueous solution of nitrate of silver, it produces a constant electric current, but always acts the part of *platinum*. This is perhaps most striking with mercury and copper, because of the marked contrast it affords to the effects produced in dilute sulphuric acid and most ordinary solutions. The constancy of the current even causes crystals of silver to form on the iron as the negative electrode. It might at first seem surprising that the power which tends to reduce silver on the iron negative electrode did not also bring back the iron from its peculiar state, whether that be a state of oxidation or not. But it must be remembered that the moment a particle of silver is reduced on the iron, it not only tends to keep the iron in the peculiar state according to the facts before described, but also acts as the negative electrode; and there is no doubt that the current of electricity which continues to circulate through the solution passes essentially between it and the silver, and not between it and the iron, the latter metal being merely the conductor interposed between the silver and the copper extremities of the metallic arrangement.

I am afraid you will think I have pursued this matter to a greater length than it deserves; but I have been exceedingly interested by M. Schoenbein's researches, and cannot help thinking that the peculiar condition of iron which he has pointed out will (whatever it may depend upon) enable us hereafter more closely to examine the surface-action of the metals and electrolytes when they are associated in voltaic combinations, and so give us a just knowledge of the nature of the two modes of action by which particles under the influence of the same power can produce either local effects of combination or current affinity[1].

I am, my dear Phillips, very truly yours,

Royal Institution, June 16, 1836. M. Faraday.

Letter from Mr. Faraday *to* Mr. Brayley *on some former Researches relative to the peculiar Voltaic Condition of Iron reobserved by Professor* Schoenbein, *supplementary to a Letter to* Mr. Phillips, *in the last Number*[2].

My dear Sir, Royal Institution, July 8, 1836.

I am greatly your debtor for having pointed out to me Sir John F. W. Herschel's paper on the action of nitric acid on iron in the Annales de Chimie et de Physique; I read it at the time of its publication, but it had totally escaped my memory, which is indeed a very bad one now. It renders one half of my letter (supplementary to Professor Schoenbein's) in the last Number of the Philosophical Magazine, page 57 (or page 239 of this volume), superfluous; and I regret only that it did not happen to be recalled to my attention in time for me to rearrange my remarks, or at all events to add to them an account of Sir John Herschel's results. However, I hope the Editors of the Phil. Mag. will allow my present letter a place in the next Number; and entertaining that hope I shall include in it a few references to former results bearing upon the extraordinary character of iron to which M. Schoenbein has revived the attention of men of science.

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[1] Exp. Researches, Eighth Series, 947. 996.

[2] Lond. and Edinb. Phil. Mag., 1836, vol. ix. p. 122.

"Bergman relates that upon adding iron to a solution of silver in the nitrous acid no precipitation ensued[1]."

Keir, who examined this action in the year 1790[2], made many excellent experiments upon it. He observed that the iron acquired a *peculiar or altered* state in the solution of silver; that this state was only superficial; that when so altered it was inactive in nitric acid; and that when ordinary iron was put into strong nitric acid there was no action, but the metal assumed the *altered* state.

Westlar, whose results I know only from the Annales des Mines for 1832[3], observed that iron or steel which had been plunged into a solution of nitrate of silver lost the power of precipitating copper from its solutions; and he attributes the effect to the assumption of a negative electric state by the part immersed, the other part of the iron having assumed the positive state.

Braconnot in 1833[4] observed, that filings or even plates of iron in strong nitric acid are not at all affected at common temperatures, and scarcely even at the boiling-point.

Sir John Herschel's observations are in reality the first which refer these phenomena to electric forces; but Westlar's, which do the same, were published before them. The results obtained by the former, extracted from a private journal dated August 1825, were first published in 1833[1]. He describes the action of nitric acid on iron; the altered state which the metal assumes; the superficial character of the change; the effect of the contact of other metals in bringing the iron back to its first state; the power of platina in assisting to bring on the altered or prepared state; and the habits of steel in nitric acid: he attributes the phenomena to a certain *permanent electric state of the surface of the metal.* I should recommend the republication of this paper in the Philosophical Magazine.

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[1] Phil. Trans. 1790, p. 374.

[2] Ibid. pp. 374, 379.

[3] Annales des Mines, 1832, vol. ii. p. 322; or Mag. de Pharm. 1830.

[4] Annales de Chimie et de Physique, vol. lii. p. 288.

Professor Daniell, in his paper on Voltaic Combinations[2] (Feb. 1836), found that on associating iron with platina in a battery charged with nitro-sulphuric acid, the iron would not act as the generating metal, and that when it was afterwards associated with zinc it acted more powerfully than platina itself. He considers the effect as explicable upon the idea of a force of heterogeneous attraction existing between bodies, and is inclined to believe that association with the platina cleanses the surface of the iron, or possibly causes a difference in the mechanical structure developed in this particular position.

In my letter, therefore, as published in the Philosophical Magazine for the present month (July), what relates to the preserving power of platina on iron ought to be struck out, as having been anticipated by Sir John Herschel, and also much of what relates to the action of silver and iron, as having been formerly recorded by Keir. The facts relating to gold and carbon in association with iron; the experimental results as to the electric currents produced; the argument respecting the chemical source of electricity in the voltaic pile; and my opinion of the cause of the phenomena as due to a relation of the superficial particles of the iron to oxygen, are what remain in the character of contributions to our knowledge of this very beautiful and important case of voltaic condition presented to us by the metal iron.

I am, my dear Sir, yours very truly,

E. W. Brayley, Esq. M. Faraday.
London Institution.

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[1] Annales de Chimie et de Physique, 1833, vol. liv. p. 87.

[2] Phil. Trans. 1836, p. 114.

A Letter to Prof. Faraday, *on certain Theoretical Opinions. By* R. HARE, *M.D., Professor of Chemistry in the University of Pennsylvania*[1].

DEAR SIR,

1. I HAVE been indebted to your kindness for several pamphlets comprising your researches in electricity, which I have perused with the greatest degree of interest.

2. You must be too well aware of the height at which you stand, in the estimation of men of science, to doubt that I entertain with diffidence any opinion in opposition to yours. I may say of you as in a former instance of Berzelius, that you occupy an elevation inaccessible to unjustifiable criticism. Under these circumstances, I hope that I may, from you, experience the candour and kindness which were displayed by the great Swedish chemist in his reply to my strictures on his nomenclature.

3. I am unable to reconcile the language which you hold in paragraph 1615, with the fundamental position taken in 1165. Agreeably to the latter, you believe ordinary induction to be the action of *contiguous* particles, consisting of a species of polarity, instead of being an action of either particles or masses at "*sensible distances.*" Agreeably to the former, you conceive that "assuming that a perfect vacuum was to intervene in the course of the line of inductive action, it does not follow from this theory that the line of particles on opposite sides of such a vacuum would not act upon each other." Again, supposing "it possible for a positively electrified particle to be in the centre of a vacuum an inch in diameter, nothing in my present view forbids that the particle should act at a distance of half an inch on all the particles forming the inner superficies of the bounding sphere."

4. Laying these quotations before you for reconsideration, I beg leave to inquire how a positively excited particle, situated as above described, can react "inductrically" with any particles

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[1] From Silliman's American Journal of Science and Arts, vol. 38, No. 1., or Phil. Mag. 1840, vol. xvii. p. 44.

[We have taken the liberty of numbering the paragraphs of Dr. Hare's letter.—EDIT.]

in the superficies of the surrounding sphere, if this species of reaction require that the particles between which it takes place be contiguous. Moreover if induction be not "an action either of particles or masses at *sensible* distances," how can a particle, situated as above described, "*act at the distance of half an inch on all the particles forming the disk of the inner superficies of the bounding sphere?*" What is a sensible distance, if half an inch is not?

5. How can the force thus exercised obey the "well-known law of the squares of the distances," if as you state (1375) the rarefaction of the air does not alter the intensity of the inductive action? In proportion as the air is rarefied, do not its particles become more remote?

6. Can the ponderable particles of a gas be deemed contiguous, in the true sense of this word, under any circumstances? And it may be well here to observe, that admitting induction to arise from an affection of intervening ponderable atoms, it is difficult to conceive that the intensity of this affection will be inversely as their number, as alleged by you. No such law holds good in the communication of heat. The air in contact with a surface at a constant elevation of temperature, such for instance as might be supported by boiling water, would not become hotter by being rarefied, and consequently could not become more efficacious in the conduction of heat from the heated surface to a colder one in its vicinity.

7. As soon as I commenced the perusal of your researches on this subject, it occurred to me that the passage of electricity through a vacuum, or a highly rarefied medium, as demonstrated by various experiments, and especially those of Davy, was inconsistent with the idea that ponderable matter could be a necessary agent in the process of electrical induction. I therefore inferred that your efforts would be primarily directed to a re-examination of that question.

8. If induction, in acting through a vacuum, be propagated in right lines, may not the curvilinear direction which it pursues, when passing through "dielectrics," be ascribed to the modifying influence which they exert?

9. If, as you concede, electrified particles on opposite sides of a vacuum can act upon each other, wherefore is the received theory of the mode in which the excited surface of a Leyden jar

induces in the opposite surface a contrary state, objectionable?

10. As the theory which you have proposed gives great importance to the idea of polarity, I regret that you have not defined the meaning which you attach to this word. As you designate that to which you refer, as a "species of polarity," it is presumable that you have conceived of several kinds with which ponderable atoms may be endowed. I find it difficult to conceive of any kind which may be capable of as many degrees of intensity as the known phenomena of electricity require; especially according to your opinion that the only difference between the fluid evolved by galvanic apparatus and that evolved by friction, is due to opposite extremes in quantity and intensity; the intensity of electrical excitement producible by the one being almost infinitely greater than that which can be produced by the other. What state of the poles can constitute quantity—what other state intensity, the same matter being capable of either electricity, as is well known to be the fact? Would it not be well to consider how, consistently with any conceivable polarization, and without the assistance of some imponderable matter, any great difference of intensity in inductive power can be created?

11. When by friction the surface is polarized so that particles are brought into a state of constraint from which they endeavour to return to their natural state, if nothing be superadded to them, it must be supposed that they have poles capable of existing in two different positions. In one of these positions, dissimilar poles coinciding, are neutralized; while in the other position, they are more remote, and consequently capable of acting upon other matter.

12. But I am unable to imagine any change which can admit of gradations of intensity, *increasing* with remoteness. I cannot figure to myself any reaction which increase of distance would not lessen. Much less can I conceive that such extremes of intensity can be thus created, as those of which you consider the existence as demonstrated. It may be suggested that the change of polarity produced in particles of electrical inductions, may arise from the forced approximation by reciprocally repellent poles, so that the intensity of the inductive force, and of their effort to return to their previous situation,

may be susceptible of the gradation which your electrical doctrines require. But could the existence of such a repellent force be consistent with the mutual cohesion which appears almost universally to be a property of ponderable particles? I am aware that, agreeably to the ingenious hypothesis of Mossotti[1], repulsion is an inherent property of the particles which we call ponderable; but then he assumes the existence of an imponderable fluid to account for cohesion; and for the necessity of such a fluid to account for induction it is my ultimate object to contend. I would suggest that it can hardly be expedient to ascribe the phenomena of electricity to the polarization of ponderable particles, unless it can be shown, that if admitted, it would be competent to produce all the known varieties of electric excitement, whether as to its nature or energy.

13. If I comprehend your theory, the opposite electrical state induced on one side of a coated pane, when the other is directly electrified, arises from an affection of the intervening vitreous particles, by which a certain polar state caused on one side of the pane, induces an opposite state on the other side. Each vitreous particle having its poles severally in opposite states, they are arranged as magnetized iron filings in lines; so that alternately opposite poles are presented in such a manner that all of one kind are exposed at one surface, and all of the other kind at the other surface. Agreeably to this or any other imaginable view of the subject, I cannot avoid considering it inevitable that each particle must have at least two poles. It seems to me that the idea of polarity requires that there shall be in any body possessing it, two opposite poles. Hence you correctly allege, that agreeably to your views it is impossible to charge a portion of matter with one electric force without the other. (See par. 1177.) But if all this be true, how can there be a "positively excited particle?" (See par. 1616.) Must not every particle be excited negatively, if it be excited positively? Must it not have a negative, as well as a positive pole?

14. I cannot agree with you in the idea, that consistently with the theory which ascribes the phenomena of electricity to

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[1] [See Scientific Memoirs, vol. i. p. 448.—EDIT.]

one fluid, there can ever be an isolated existence either of the positive or negative state. Agreeably to this theory, any excited space, whether minus or plus, must have an adjoining space relatively in a different state. Between the phenomena of positive and negative excitement there will be no other distinction than that arising from the direction in which the fluid will endeavour to move. If the excited space be positive, it must strive to flow outward; if negative, it will strive to flow inward. When sufficiently intense, the direction will be shown by the greater length of the spark, when passing from a small ball to a large one. It is always longer when the small ball is positive, and the large one negative, than when their positions are reversed[1].

15. But for any current it is no less necessary that the pressure should be on one side, comparatively minus, than that on the other side, it should be comparatively plus; and this state of the forces must exist whether the current originates from a hiatus before or from pressure behind. One current cannot differ essentially from another, however they may be produced.

16. In paragraph 1330, I have been struck with the following query, "What then is to separate the principle of these extremes, perfect conduction and perfect insulation, from each other; since the moment we leave the smallest degree of perfection at either extremity, we involve the element of perfection at the opposite ends?" Might not this query be made with as much reason in the case of motion and rest, between the extremes of which there is an infinity of gradations? If we are not to confound motion with rest, because in proportion as the former is retarded, it differs less from the latter; wherefore should we confound insulation with conduction, because in proportion as the one is less efficient, it becomes less remote from the other?

17. In any case of the intermixture of opposite qualities, may it not be said in the language which you employ, "the moment we leave the element of perfection at one extremity, we involve the element of perfection at the opposite"? Might it not be

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[1] See my Essay on the causes of the diversity in the length of the sparks, erroneously distinguished as positive and negative, in vol. v. American Philosophical Transactions.

said of light and darkness, or of opakeness and translucency? in which case, to resort to your language again, it might be added, "especially as we have not in nature a case of perfection at one extremity or the other." But if there be not in nature any two bodies, of which one possesses the property of perfectly resisting the passage of electricity, while the other is endowed with the faculty of permitting its passage without any resistance; does this affect the propriety of considering the qualities of *insulation* and conduction in the abstract, as perfectly distinct, and inferring that so far as matter may be endowed with the one property, it must be wanting in the other?

18. Have you ever known electricity to pass through a pane of sound glass. My knowledge and experience create an impression that a coated pane is never discharged through the glass unless it be cracked or perforated. That the property by which glass resists the passage of electricity, can be confounded with that which enables a metallic wire to permit of its transfer, agreeably to Wheatstone's experiments, with a velocity greater than that of the solar rays, is to my mind inconceivable.

19. You infer that the residual charge of a battery arises from the partial penetration of the glass by the opposite excitements. But if glass be penetrable by electricity, why does it not pass through it without a fracture or perforation?

20. According to your doctrine, induction consists "in a forced state of polarization in contiguous rows of the particles of the glass" (1300); and since this is propagated from one side to the other, it must of course exist equally at all depths. Yet the partial penetration suggested by you, supposes a collateral affection of the same kind, extending only to a limited depth. Is this consistent? Is it not more reasonable to suppose that the air in the vicinity of the coating gradually relinquishes to it a portion of free electricity, conveyed into it by what you call "*convection*"? The coating being equally in contact with the air and glass, it appears to me more easy to conceive that the air might be penetrated by the excitement, than the glass.

21. In paragraph 1300, I observe the following statement: "*When a Leyden jar is charged, the particles of the glass are forced into this polarized and constrained condition by the electricity of the charging apparatus. Discharge is the return of the particles to their natural state, from their state of ten-*

sion, whenever the two electric forces are allowed to be disposed of in some other direction." As you have not previously mentioned any particular direction in which the forces are exercised during the prevalence of this constrained condition, I am at a loss as to what meaning I am to attach to the words "some other direction." The word *some*, would lead to the idea that there was an uncertainty respecting the direction in which the forces might be disposed of; whereas it appears to me that the only direction in which they can operate, must be the opposite of that by which they have been induced.

22. The electrified particles can only "return to their natural state" by retracing the path by which they departed from it. I would suggest that for the words "*to be disposed of in some other direction*," it would be better to substitute the following, "*to compensate each other by an adequate communication.*"

23. Agreeably to the explanation of the phenomenon of coated electrics afforded in the paragraph above quoted (1300), by what process can it be conceived that the opposite polarization of the surfaces can be neutralized by conduction through a metallic wire? If I understand your hypothesis correctly, the process by which the polarization of one of the vitreous surfaces in a pane produces an opposite polarization in the other, is precisely the same as that by which the electricity applied to one end of the wire extends itself to the other end.

24. I cannot conceive how two processes severally producing results so diametrically opposite as insulation and conduction, can be the same. By the former, a derangement of the electric equilibrium may be permanently sustained, while by the other, all derangement is counteracted with a rapidity almost infinite. But if the opposite charges are dependent upon a polarity induced in contiguous atoms of the glass, which endures so long as no communication ensues between the surfaces; by what conceivable process can a perfect conductor cause a discharge to take place, with a velocity at least as great as that of the solar light? Is it conceivable that all the lines of "contra-induction" or depolarization can concentrate themselves upon the wire from each surface so as to produce therein an intensity of polarization proportioned to the concentration; and that the opposite forces resulting from the polarization are thus reciprocally compensated? I must confess, such a concentra-

tration of such forces or states, is to me difficult to reconcile with the conception that it is at all to be ascribed to the action of rows of *contiguous ponderable particles.*

25. Does not your hypothesis require that the metallic particles, at opposite ends of the wire, shall in the first instance be subjected to the same polarization as the excited particles of the glass; and that the opposite polarizations, transmitted to some intervening point, should thus be mutually destroyed, the one by the other? But if discharge involves a return to the same state in vitreous particles, the same must be true in those of the metallic wire. Wherefore then are these dissipated, when the discharge is sufficiently powerful? Their dissipation must take place either while they are in the state of being polarized, or in that of returning to their natural state. But if it happen when in the first-mentioned state, the conductor must be destroyed before the opposite polarization upon the surfaces can be neutralized by its intervention. But if not dissipated in the act of being polarized, is it reasonable to suppose that the metallic particles can be sundered by returning to their *natural state* of polarization?

26. Supposing that ordinary electrical induction could be satisfactorily ascribed to the reaction of ponderable particles, it cannot, it seems to me, be pretended that magnetic and electro-magnetic induction is referable to this species of reaction. It will be admitted that the Faradian currents do not for their production require intervening ponderable atoms.

27. From a note subjoined to page 37 of your pamphlet[1], it appears that "on the question of the existence of one or more imponderable fluids as the cause of electrical phenomena, it has not been your intention to decide." I should be much gratified if any of the strictures in which I have been so bold as to indulge, should contribute to influence your ultimate decision.

28. It appears to me that there has been an undue disposition to burden the matter, usually regarded as such, with more duties than it can perform. Although it is only with the properties of matter that we have a direct acquaintance, and the existence of matter rests upon a theoretical inference that since we perceive properties, there must be material particles to which those properties belong; yet there is no conviction which the

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[1] Page 409 of the former volume of these papers.

mass of mankind entertain with more firmness than that of the existence of matter in that ponderable form, in which it is instinctively recognised by people of common sense. Not perceiving that this conviction can only be supported as a theoretic deduction from our perception of the properties; there is a reluctance to admit the existence of other matter, which has not in its favour the same instinctive conception, although theoretically similar reasoning would apply. But if one kind of matter be admitted to exist because we perceive properties, the existence of which cannot be otherwise explained, are we not warranted, if we notice more properties than can reasonably be assigned to one kind of matter, to assume the existence of another kind of matter?

29. Independently of the considerations which have heretofore led some philosophers to suppose that we are surrounded by an ocean of electric matter, which by its redundancy or deficiency is capable of producing the phenomena of mechanical electricity, it has appeared to me inconceivable that the phenomena of galvanism and electro-magnetism, latterly brought into view, can be satisfactorily explained without supposing the agency of an intervening imponderable medium by whose subserviency the inductive influence of currents or magnets is propagated. If in that wonderful reciprocal reaction between masses and particles, to which I have alluded, the polarization of condensed or accumulated portions of intervening imponderable matter, can be brought in as a link to connect the otherwise imperfect chain of causes; it would appear to me a most important instrument in lifting the curtain which at present hides from our intellectual vision, this highly important mechanism of nature.

30. Having devised so many ingenious experiments tending to show that the received ideas of electrical induction are inadequate to explain the phenomena without supposing a modifying influence in intervening ponderable matter, should there prove to be cases in which the results cannot be satisfactorily explained by ascribing them to ponderable particles, I hope that you may be induced to review the whole ground, in order to determine whether the part to be assigned to contiguous ponderable particles, be not secondary to that performed by the imponderable principles by which they are surrounded.

31. But if galvanic phenomena be due to ponderable (*imponderable*?) matter, evidently that matter must be in a state of combination. To what other cause than an intense affinity between it and the metallic particles with which it is associated, can its confinement be ascribed consistently with your estimate of the enormous quantity which exists in metals? If "a grain of water, or a grain of zinc, contain as much of the electric fluid as would supply eight hundred thousand charges of a battery containing a coated surface of fifteen hundred square inches," how intense must be the attraction by which this matter is confined! In such cases may not the material cause of electricity be considered as latent, agreeably to the suggestion of Œrsted, the founder of electro-magnetism? It is in combination with matter, and only capable of producing the appropriate effects of voltaic currents when in act of transfer from combination with one atom to another; this transfer being at once an effect and a cause of chemical decomposition, as you have demonstrated.

32. If polarization in any form can be conceived to admit of the requisite gradations of intensity, which the phenomena seem to demand; would it not be more reasonable to suppose that it operates by means of an imponderable fluid existing throughout all space, however devoid of other matter? May not an electric current, so called, be a progressive polarization of rows of the electric particles, the polarity being produced at one end and destroyed at the other incessantly, as I understood you to suggest in the case of contiguous ponderable atoms?

33. When the electric particles within different wires are polarized in the same tangential direction, the opposite poles being in proximity, there will be attraction. When the currents of polarization move oppositely, similar poles coinciding, there will be repulsion. The phenomena require that the magnetized or polarized particles should be arranged as tangents to the circumference, not as radii to the axis. Moreover, the progressive movement must be propagated in spiral lines in order to account for rotary influence.

34. Between a wire which is the mean of a galvanic discharge and another not making a part of a circuit, the electric matter which intervenes may, by undergoing a polarization,

become the medium of producing a progressive polarization in the second wire moving in a direction opposite to that in the inducing wire; or in other words, an electrical current of the species called Faradian may be generated.

35. By progressive polarization in a wire, may not stationary polarization or magnetism be created; and reciprocally by magnetic polarity may not progressive polarization be excited?

36. Might not the difficulty, above suggested, of the incompetency of any imaginable polarization to produce all the varieties of electrical excitement which facts require for explanation, be surmounted by supposing intensity to result from an accumulation of free electric polarized particles, and quantity from a still greater accumulation of such particles, polarized in a latent state or in chemical combination?

37. There are it would seem many indications in favour of the idea that electric excitement may be due to a forced polarity, but in endeavouring to define the state thus designated, or to explain by means of it the diversities of electrical charges, currents and effects, I have always felt the incompetency of any hypothesis which I could imagine. How are we to explain the insensibility of a gold-leaf electroscope to a galvanized wire, or the indifference of a magnetic needle to the most intensely electrified surfaces?

38. Possibly the Franklinian hypothesis may be combined with that above suggested, so that an electrical current may be constituted of an imponderable fluid in a state of polarization, the two electricities being the consequence of the position of the poles, or their presentation. Positive electricity may be the result of an accumulation of electric particles, presenting poles of one kind; negative, from a like accumulation of the same matter with a presentation of the opposite poles, inducing of course an opposite polarity. The condensation of the electric matter, within ponderable matter, may vary in obedience to a property analogous to that which determines the capacity for heat, and the different influence of dielectrics upon the process of electrical induction may arise from this source of variation.

With the highest esteem, I am yours truly,

Robert Hare.

An Answer to Dr. Hare's *Letter on certain Theoretical Opinions.*

My dear Sir,

i. Your kind remarks have caused me very carefully to revise the general principles of the view of *static induction* which I have ventured to put forth, with the very natural fear that as it did not obtain your acceptance, it might be founded in error; for it is not a mere complimentary expression when I say I have very great respect for your judgement. As the reconsideration of them has not made me aware that they differ amongst themselves or with facts, the resulting impression on my mind is, that I must have expressed my meaning imperfectly, and I have a hope that when more clearly stated my words may gain your approbation. I feel that many of the words in the language of electrical science possess much meaning; and yet their interpretation by different philosophers often varies more or less, so that they do not carry exactly the same idea to the minds of different men: this often renders it difficult, when such words force themselves into use, to express with brevity as much as, and no more than, one really wishes to say.

ii. My theory of induction (as set forth in Series xi. xii. and xiii.) makes no assertion as to the nature of electricity, or at all questions any of the theories respecting that subject (1667.). It does not even include the origination of the developed or excited state of the power or powers; but taking that as it is given by experiment and observation, it concerns itself only with the arrangement of the force in its communication to a distance in that particular yet very general phenomenon called *static induction* (1668.). It is neither the nature nor the amount of the force which it decides upon, but solely its mode of distribution.

iii. Bodies whether conductors or non-conductors can be *charged.* The word *charge* is equivocal: sometimes it means that state which a glass tube acquires when rubbed by silk, or which the prime conductor of a machine acquires when the latter is in action; at other times it means the state of a Leyden jar or similar inductive arrangement when it is said to be charged. In the first case the word means only the peculiar condition of an electrified mass of matter considered by itself, and does not apparently involve the idea of induction; in the second it means the whole of the relations of two such masses charged in opposite states, and most intimately connected by inductive action.

iv. Let three insulated metallic spheres A, B and C be placed in a line, and not in contact: let A be electrified positively, and then C uninsulated; besides the general action of the whole system upon all surrounding matter, there will occur a case of inductive action amongst the three balls, which may be considered apart, as the type and illustration of the whole of my theory: A will be charged positively; B will acquire the negative state at the surface towards A, and the positive state at the surface furthest from it; and C will be charged negatively.

v. The ball B will be in what is often called a polarized condition, *i. e.* opposite parts will exhibit the opposite electrical states, and the two sums of these opposite states will be exactly equal to each other. A and C will not be in this polarized state, for they will each be, as it is said, charged (iii.), the one positively, the other negatively, and they will present no polarity as far as this particular act of induction (iv.) is concerned.

vi. That one part of A is more positive than another part does not render it polar in the sense in which that word has just been used. We are considering a particular case of induction, and have to throw out of view the states of those parts not under the inductive action. Or if any embarrassment still arise from the fact that A is not uniformly charged all over, then we have merely to surround it with balls, such as B and C, on every side, so that its state shall be alike on every part of its surface (because of the uniformity of its inductive influence in all directions) and then that difficulty will be removed. A therefore is charged, but not polarly; B assumes a polar condition; and C is charged inducteously (1483.), being by the prime influence of A brought into the opposite or negative electrical state through the intervention of the intermediate and polarized ball B.

vii. Simple charge therefore does not imply polarity in the body charged. Inductive charge (applying that term to the sphere B and all bodies in a similar condition (v.)) does (1672.). The word charge as applied to a Leyden jar, or to the *whole* of any inductive arrangement, by including *all* the effects, comprehends of course both these states.

viii. As another expression of my theory, I will put the following case. Suppose a metallic sphere C, formed of a thin shell a foot in diameter; suppose also in the centre of it another metallic sphere A only an inch in diameter; suppose the central sphere A charged positively with electricity to the amount we will say of 100; it would act by induction through the air, lac, or other insulator between it and the large sphere C; the interior of the latter would be negative, and its exterior positive, and the sum of the positive force upon the whole of the external surface would be 100. The sphere C would in fact be polarized (v.) as regards its inner and outer surfaces.

ix. Let us now conceive that instead of mere air, or other insulating dielectric, within C between it and A, there is a thin metallic concentric sphere B six inches in diameter. This will make no difference in the ultimate result, for the charged ball A will render the inner and outer surfaces of this sphere B negative and positive, and it again will render the inner and outer surfaces of the large sphere C negative and positive, the sum of the positive forces on the outside of C being still 100.

x. Instead of one intervening sphere let us imagine 100 or 1000 concentric with each other, and separated by insulating matter, still the same final result will occur; the central ball will act inductrically, the influence originating with it will be carried on from sphere to sphere, and positive force equal to 100 will appear on the outside of the external sphere.

xi. Again, imagine that all these spheres are subdivided into myriads of particles, each being effectively insulated from its neighbours (1679.), still the same final result will occur; the inductric body A will polarize all these, and having its influence carried on by them in their newly acquired state, will exert precisely the same amount of action on the external sphere C as before, and positive force equal to 100 will appear on its outer surface.

xii. Such a state of the space between the inductric and inducteous surfaces represents what I believe to be the state of an insulating dielectric under inductive influence; the particles of which by the theory are assumed to be conductors individually, but not to one another (1669.).

xiii. In asserting that 100 of positive force will appear on the outside of the external sphere under all these variations, I presume I am saying no more than what every electrician will admit. Were it not so, then positive and negative electricities could exist by themselves, and without relation to each other (1169. 1177.), or they could exist in proportions not equivalent to each other. There are plenty of experiments, both old and new, which prove the truth of the principle, and I need not go further into it here.

xiv. Suppose a plane to pass through the centre of this spherical system, and conceive that instead of the space between the central ball A and the external sphere C being occupied by a uniform distribution of the equal metallic particles, three times as many were grouped in the one half to what occurred in the other half, the insulation of the particles being always preserved: then more of the inductric influence of A would be conveyed outwards to the inner surface of the sphere C, through that half of the space where the greater number of metallic particles existed, than through the other half: still the exterior of the outer sphere C would be uniformly charged with positive electricity, the amount of which would be 100 as before.

xv. The actions of the two portions of space, as they have just been supposed to be constituted (xiv.), is as if they possessed two different *specific inductive capacities* (1296.); but I by no means intend to say, that *specific inductive capacity* depends in all cases upon the number of conducting particles of which the dielectric is formed, or upon their vicinity. The full cause of the evident difference of inductive capacity of different bodies is a problem as yet to be solved.

xvi. In my papers I speak of all induction as being dependent on the action of contiguous particles, *i. e.* I assume that insulating bodies consist of particles which are conductors individually (1669.), but do not conduct to each other provided the intensity of action to which they are subject is beneath a given amount (1326. 1674. 1675.); and that when the inductric body acts upon conductors at a distance, it does so by polarizing (1298. 1670.) all those particles which occur in the portion of dielectric between it and them. I have used the term *contiguous* (1164. 1673.), but have I hope sufficiently expressed the meaning I attach to it; first by saying at par. 1615, "the next existing particle being considered as the contiguous one"; then in a note to par. 1665, by the words, "I mean by contiguous particles those which are next to each other, not that there is no space between them;" and further by the note to par. 1164. of the octavo edition of my Researches, which is as follows: "The word contiguous is perhaps not the best that might have been used here and elsewhere, for as particles do not touch each other it is not strictly correct. I was induced to employ it because in its common acceptation it enabled me to state the theory plainly and with facility. By contiguous particles, I mean those which are next."

xvii. Finally, my reasons for adopting the molecular theory of induction were the phenomena of electrolytic discharge (1164. 1343.), of induction in curved lines (1166. 1215.), of specific inductive capacity (1167. 1252.), of penetration and return action (1245.), of difference of conduction and insulation (1320.), of polar forces (1665.), &c. &c., but for these reasons and any strength or value they may possess I refer to the papers themselves.

xviii. I will now turn to such parts of your critical remarks as may require attention. A man who advances what he thinks to be new truths, and to develope principles which profess to be more consistent with the laws of nature than those already in the field, is liable to be charged, first with self-contradiction; then with the contradiction of facts; or he may be obscure in his expression, and so justly subject to certain queries; or he may be found in non-agreement with the opinions of others. The first and second points are very important, and every one subject to such charges must be anxious to be made aware of, and also to set himself free from or acknowledge them; the third is also a fault to be removed if possible; the fourth is a matter of but small consequence in comparison with the other three; for as every man who has the courage, not to say rashness, of forming an opinion of his own, thinks it better than any from which he differs, so it is only deeper investigation, and most generally future investigators, who can decide which is in the right.

xix. I am afraid I shall find it rather difficult to refer to your letter. I will, however, reckon the paragraphs in order from the top of each page, considering that the first which has its *beginning* first in the page[1]. In referring to my own matter I will employ the usual figures for the paragraphs of the Experimental Researches, and small Roman numerals for those of this communication.

xx. At paragraph 3, you say, you cannot reconcile my language at 1615, with that at 1165. In the latter place I have said I believe *ordinary induction* in all cases to be an action of *contiguous* particles, and in the former assuming a very hypothetical case, that of a vacuum, I have said nothing in my theory forbids that a charged particle in the centre of a vacuum should act on the particle next to it, though that should be half an inch off. With the meaning which I have carefully attached to the word contiguous (xvi.) I see no contradiction here in the terms used, nor any natural impossibility or improbability in such an action. Nevertheless all *ordinary* induction is to me an action of contiguous particles, being particles at insensible distances: induction across a vacuum is not an ordinary instance, and yet I do not perceive that it cannot come under the same principles of action.

xxi. As an illustration of my meaning, I may refer to the case, parallel with mine, as to the extreme difference of interval between the acting particles or bodies, of the modern views of the radiation and conduction of heat. In radiation the rays leave the hot particles and pass occasionally through great distances to the next particle, fitted to receive them: in conduction, where the heat passes from the hotter particles to those which are contiguous and form part of the same mass, still the passage is considered to be by a process precisely like that of radiation; and though the effects are, as is well known, extremely different in their appearance, it cannot as yet be shown that the principle of communication is not the same in both.

xxii. So on this point respecting contiguous particles and induction across half an inch of vacuum, I do not see that I am in contradiction with myself or with any natural law or fact.

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[1] We shall change Prof. Faraday's references for the numbers which we have attached to Dr. Hare's letter, and refer thus, par. 23, &c.—Ed. Phil. Mag.

xxiii. Paragraph 4 is answered by the above remarks and by viii. ix. x.

xxiv. Paragraph 5 is answered according to my theory by viii. ix. x. xi. xii. and xiii.

xxv. Paragraph 6 is answered, except in the matter of opinion (xviii.), according to my theory by xvi. The conduction of heat referred to in the paragraph itself, will, as it appears to me, bear no comparison with the phenomenon of electrical induction:—the first refers to the distant influence of an agent which travels by a very slow process, the second to one where distant influence is simultaneous, so to speak, with the origin of the force at the place of action:—the first refers to an agent which is represented by the idea of one imponderable fluid, the second to an agency better represented probably by the idea of two fluids, or at least by two forces:—the first involves no polar action, nor any of its consequences, the second depends essentially on such actions;—with the first, if a certain portion be originally employed in the centre of a spherical arrangement, but a small part appears ultimately at the surface; with the second, an amount of force appears instantly at the surface (viii. ix. x. xi. xii. xiii. xiv.) exactly equal to the exciting or moving force, which is still at the centre.

xxvi. Paragraph 13 involves another charge of self-contradiction, from which, therefore, I will next endeavour to set myself free. You say I " correctly allege that it is impossible to charge a portion of matter with one electric force without the other (see par. 1177). But if all this be true, how can there be a *positively excited particle*? (see par. 1616). Must not every particle be excited negatively if it be excited positively? Must it not have a negative as well as a positive pole?" Now I have not said exactly what you attribute to me; my words are, " it is impossible, experimentally, to charge a portion of matter with one electric force *independently* of the other: charge always implies *induction*, for it can in no instance be effected without (1177.)." I can, however, easily perceive how my words have conveyed a very different idea to your mind, and probably to others, than that I meant to express.

xxvii. Using the word *charge* in its simplest meaning (iii. iv.), I think that a body *can* be charged with one electric force without the other, that body being considered in relation to itself only. But I think that such charge cannot exist without induction (1178.), or independently of what is called the development of an equal amount of the other electric force, not in itself, but in the neighbouring consecutive particles of the surrounding dielectric, and through them of the facing particles of the uninsulated surrounding conducting bodies, which, under the circumstances, terminate as it were the particular case of induction. I have no idea, therefore, that a particle when charged must itself of necessity be polar; the spheres A B C of iv., v., vi., vii., fully illustrate my views (672.).

xxviii. Paragraph 20 includes the question, " is this consistent?" implying self-contradiction, which, therefore, I proceed to notice. The question arises out of the possibility of glass being a (slow) conductor or not of electricity, a point questioned also in the two preceding paragraphs. I believe that it is. I have charged small Leyden jars made of thin flint glass tube with electricity, taken out the charging wires, sealed them up hermetically, and after two and three years have opened and found no charge in them. I will refer you also to Belli's curious experiments upon the successive charges of a jar and the successive return of portions of these charges[1]. I will also refer to the experiments with the shell lac hemisphere, especially that described in 1237. of my Researches; also the experiment in 1246. I cannot conceive how, in these cases, the air in the vicinity of the coating could gradually relinquish to it a portion of free electricity, conveyed into it by what I called convection, since in the first experiment quoted (1237.), when the return was gradual, there was *no coating*; and in the second (1246.), when there was *a coating*, the return action was most sudden and instantaneous.

xxix. Paragraphs 21 and 22 perhaps only require a few words of explanation. In a charged Leyden jar I have considered the two opposite forces on the inductric and inducteous surfaces as being directed towards each other through the glass of the jar, provided the jar have no projection of its inner coating, and is uninsulated on the outside (1682.). When discharge by a wire or discharger, or any other of the many arrangements used for that purpose is effected, these supply the " some other directions" spoken of (1682. 1683.).

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[1] Bibliotheca Italiana, 1837, lxxxv. p. 417.

xxx. The inquiry in paragraph 23, I should answer by saying, that the process is the same as that by which the polarity of the sphere B (iv., v.,) would be neutralized if the spheres A and C were made to communicate by a metallic wire; or that by which the 100 or 1000 intermediate spheres (x.) or the myriads of polarized conducting particles (xi.) would be discharged, if the inner sphere A, and the outer one C, were brought into communication by an insulated wire; a circumstance which would not in the least affect the condition of the power on the exterior of the globe C.

xxxi. The obscurity in my papers, which has led to your remarks in paragraph 25, arises, as it appears to me (after my own imperfect expression), from the uncertain or double meaning of the word discharge. You say, "if discharge involves a return to the same state in vitreous particles, the same must be true in those of the metallic wire. Wherefore then are these dissipated when the discharge is sufficiently powerful?" A jar is said to be discharged when its charged state is reduced by any means, and it is found in its first indifferent condition. The word is then used simply to express the state of the apparatus; and so I have used it in the expressions criticised in paragraph 21, already referred to. The process of discharge, or the mode by which the jar is brought into the discharged state, may be subdivided, as of various kinds; and I have spoken of conductive (1320.), electrolytic (1343.), disruptive (1359.), and convective (1562.) discharge, any one of which may cause the discharge of the jar, or the discharge of the inductive arrangements described in this letter (xxx.), the action of the particles in any one of these cases being entirely different from the mere return action of the polarized particles of the glass of the jar, or the polarized globe B (v.), to their first state. My view of the relation of insulators and conductors, as bodies of one class, is given at 1320. 1675. &c. of the Researches; but I do not think the particles of the good conductors acquire an intensity of polarization anything like that of the particles of bad conductors; on the contrary, I conceive that the contiguous polarized particles (1670.) of good conductors discharge to each other when their polarity is at a very low degree of intensity (1326. 1338. 1675.). The question of why are the metallic particles dissipated when the charge is sufficiently powerful, is one that my theory is not called upon at present to answer, since it will be acknowledged by all, that the dissipation is not necessary to discharge. That different effects ensue upon the subjection of bodies to different degrees of the same power, is common enough in experimental philosophy; thus, one degree of heat will merely make water hot, whilst a higher degree will *dissipate* it as steam, and a lower will convert it into ice.

xxxii. The next most important point, as it appears to me, is that contained in paragraphs 16 and 17. I have said (1330.), " what then is to separate the principle of these two extremes, perfect conduction and perfect insulation, from each other, since the moment we leave in the smallest degree perfection at either extremity we involve the element of perfection at the opposite end?" and upon this you say, might not this query be made with as much reason in the case of motion and rest?—and in any case of the intermixture of opposite qualities, may it not be said, the moment we leave the element of perfection at one end, we involve the element of perfection at the opposite?—may it not be said of light and darkness, or of opakeness and translucency? and so forth.

xxxiii. I admit that these questions are very properly put; not that I go to the full extent of them all, as for instance that of motion and rest; but I do not perceive their bearing upon the question, of whether conduction and insulation are different properties, dependent upon two different modes of action of the particles of the substances respectively possessing these actions, or whether they are only differences in *degree* of one and the same mode of action? In this question, however, lies the whole gist of the matter. To explain my views, I will put a case or two. In former times a principle or force of levity was admitted, as well as of gravity, and certain variations in the weights of bodies were supposed to be caused by different combinations of substances possessing these two principles. In later times, the levity principle has been discarded; and though we still have imponderable substances, yet the phenomena causing weight have been accounted for by one force or principle only, that of gravity; the difference in the gravitation of different bodies being considered due to differences in *degree* of this *one force* resident in them all. Now no one can for a moment suppose that it is the same thing philosophically to assume either the two forces or the one force for the explanation of the phenomena in question.

xxxiv. Again, at one time there was a distinction taken between the principle of heat and that of cold: at present that theory is done away with, and the phenomena of heat and cold are referred to the same class, (as I refer those of insulation and conduction to one class,) and to the influence of different degrees of the same power. But no one can say that the two theories, namely, that including but one positive principle, and that including two, are alike.

xxxv. Again, there is the theory of one electric fluid and also that of two. One explains by the difference in degree or quantity of one fluid, what the other attributes to a variation in the quantity and relation of two fluids. Both cannot be true. That they have nearly equal hold of our assent, is only a proof of our ignorance; and it is certain, whichever is the false theory, is at present holding the minds of its supporters in bondage, and is greatly retarding the progress of science.

xxxvi. I think it therefore important, if we can, to ascertain whether insulation and conduction are cases of the same class, just as it is important to know that hot and cold are phenomena of the same kind. As it is of consequence to show that smoke ascends and a stone descends in obedience to one property of matter, so I think it is of consequence to show that one body insulates and another conducts only in consequence of a difference in degree of one common property which they both possess; and that in both cases the effects are consistent with my theory of induction.

xxxvii. I now come to what may be considered as queries in your letter which I ought to answer. Paragraph 8 contains one. As I concede that particles on opposite sides of a vacuum may perhaps act on each other, you ask, "wherefore is the received theory of the mode in which the excited surface of a Leyden jar induces in the opposite surface a contrary state, objectionable?" My reasons for thinking the excited surface does not directly induce upon the opposite surface, &c., is, first, my belief that the glass consists of particles conductive in themselves, but insulated as respects each other (xvii.); and next, that in the arrangement given iv., ix., or x., A does not induce directly on C, but through the intermediate masses or particles of conducting matter.

xxxviii. In the next paragraph, the question is rather implied than asked—what do I mean by polarity? I had hoped that the paragraphs 1669. 1670. 1671. 1672. 1679. 1686. 1687. 1688. 1699. 1700. 1701. 1702. 1703. 1704. in the Researches, would have been sufficient to convey my meaning, and I am inclined to think you had not perhaps seen them when your letter was written. They, and the observations already made (v., xxvi.), with the case given (iv., v.), will, I think, be sufficient as my answer. The sense of the word *polarity* is so diverse when applied to light, to a crystal, to a magnet, to the voltaic battery, and so different in all these cases to that of the word when applied to the state of a conductor under induction (v.), that I thought it safer to use the phrase "species of polarity," than any other, which being more expressive would pledge me further than I wished.

xxxix. Paragraph 11 involves a mistake of my views. I do not consider bodies which are charged by friction or otherwise, as polarized, or as having their particles polarized (iii., iv., xxvii.). This paragraph and the next do not require, therefore, any further remark, especially after what I have said of polarity above (xxxviii.).

xl. And now, my dear sir, I think I ought to draw my reply to an end. The paragraphs which remain unanswered refer, I think, only to differences of opinion, or else, not even to differences, but opinions regarding which I have not ventured to judge. These opinions I esteem as of the utmost importance; but that is a reason which makes me the rather desirous to decline entering upon their consideration, inasmuch as on many of their connected points I have formed no decided notion, but am constrained by ignorance and the contrast of facts to hold my judgment as yet in suspense. It is, indeed, to me an annoying matter to find how many subjects there are in electrical science, on which, if I were asked for an opinion, I should have to say, I cannot tell,—I do not know; but, on the other hand, it is encouraging to think that these are they which if pursued industriously, experimentally, and thoughtfully, will lead to new discoveries. Such a subject, for instance, occurs in the currents produced by dynamic induction, which you say it will be admitted do not require for their production intervening ponderable atoms. For my own part, I more than half incline to think they do require these intervening particles, that is, where any particles intervene (1729. 1733. 1738.). But on this question, as on many others, I have not yet made up my mind. Allow me, therefore, here to conclude my letter; and believe me to be, with the highest esteem,

My dear Sir,
Your obliged and faithful Servant,
Royal Institution, April 18, 1840. M. FARADAY.

A second letter was written by Dr. Hare to Mr. Faraday on the same subject, which may be found in the American Journal of Science, vol. xli. p. 2, or the Lond. and Edinb. Phil. Mag. 1841, vol. xviii. p. 465.

On Dr. Hare's *Second Letter, and on the Chemical and Contact Theories of the Voltaic Battery*[1].

To R. Taylor, Esq.

MY DEAR SIR,

YOU are aware that considerations regarding health have prevented me from working or reading in science for the last two years. This will account to you for my ignorance of the circumstance that you had reprinted Dr. Hare's second letter to me[2]; and I believe I knew it only for the first time a week or two ago, on beginning to read up. As some persons think a letter unanswered is also unanswerable, I write merely to say, that when it was sent to me as printed in Silliman's Journal, I sent a brief letter back, declining to enter into discussion, since I had nothing more to say than had been said, and still thought that that was sufficient to enable my own mind to rest in the view it had taken of static induction, &c. My reason for declining was no want of respect to Dr. Hare, but a strong conviction that controversial reply and rejoinder is but a vain occupation. Professor Silliman wrote me word that he had very unfortunately lost my brief note, but hoped to find it and print it[1]. Since then I have forgotten the matter, and only renew it to give the same sort of answer to the letter as contained in your Journal.

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[1] Lond. and Edinb. Phil. Mag., 1843, vol. xxiii.

[2] Ibid. 1841, vol. xviii. p. 465.

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[1] I have recently found the rough copy of this letter and venture to print it as a note.—M. F., June 1844.

Royal Institution, London, 6th May 1841.

MY DEAR SIR,

I received a week or two ago the printed copy of your second letter, dated January 1, 1841, and am greatly obliged by it, both as it is another expression of your kindness, and also an evident proof that you think the views I have ventured to put forth on Electrical Induction are worthy of notice.

You must excuse me however for several reasons from answering it at any length: the first is my distaste for controversy, which is so great, that I would on no account our correspondence should acquire that character. I have often seen it do great harm, and yet remember few cases in natural knowledge where it has helped much either to pull down error or advance truth. Criticism, on the other hand, is of much value; and when criticism, such as yours, has done its duty, then it is for other minds than those either of the author or critic to decide upon and acknowledge the right.

A second reason is, that I do not wish to be drawn into statements more precise than are my thoughts, and this I have already expressed in my former letter (xl.).

A third is, that I do not find anything in your last communication which creates any difficulty in my mind with respect to my view of electrical induction, nor any important point which is not answered by my papers generally, or by my former letter to you. In saying this, I keep in mind paragraphs i., xviii., and also xvii. and xxxi. of mine to you. Do not think however that I have the vanity to suppose that this my opinion is of any importance to the scientific world, or any answer to your letter; it is merely of consequence as giving a reason why I need not go further into the statement of that which at present appears to me already correctly stated.

The fourth reason is, that judging from what I have been able to observe, I do not perceive that the statement I have endeavoured to give of my theory leads other persons to apprehend its principles in a manner seriously different from that which I should desire; and such being the case, I can have nothing more to say, since they are the judges, and have the evidence as fairly before them as present facts and circumstances permit.

I am, my dear Sir, your obliged and faithful Servant,

Dr. Hare, &c. &c. &c. M. FARADAY.

DEAR SIR,

Will you let me trouble you with the above for your Journal, as my answer to Dr. Hare's second letter to me?

Ever your obliged Servant,

Professor Silliman, &c. &c. &c. M. FARADAY.

I perceive also in your Magazine several attacks, from Germany, Italy and Belgium, upon the chemical theory of the voltaic battery, and some of them upon experiments of mine. For my own part I refrain from publicly noticing these arguments, simply because there is nothing in them which suggests to my mind a new thought illustrative of the subject, or gives any ground for a change in my opinion. But whilst speaking on this point I cannot help expressing a wish that some of the advocates of the contact theory would touch upon the consideration which, up to this time, seems to have been most carefully avoided, namely the unphilosophical nature of the assumed contact force, as I have endeavoured to express it in par. 2065 to 2073 of my "Experimental Researches," and as Dr. Roget has expressed it in words which I have appended in a note to my paper. Such a consideration seems to me to remove the *foundation itself* of the contact theory. I wish you could be persuaded to think it worth while to reprint those three pages in your Magazine[1]. As far as I can perceive, they express a fundamental principle which cannot be set aside or evaded by a philosophical mind possessing only a moderate degree of strictness in its reasonings; and I must confess, that until some answer, or some show of answer in the form of assumption or otherwise, is made to that expression of what I believe to be a law of nature, I shall feel very little inclined to attach much importance to facts which, though urged in favour of the contact theory, are ever found by the partisans of the chemical theory just as favourable to, and consistent with, their peculiar views.

I am, my dear Sir,
Very faithfully yours,
M. Faraday.

Royal Institution, March 11, 1843.

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[1] We purpose to insert these pages in our next Number.—Ed. Phil. Mag.

On some supposed forms of Lightning[1].

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

THE magnificent display of lightning which we had on the evening of the 27th of last month, and its peculiar appearance to crowds of observers at London, with the consequent impressions on their minds, induce me to trouble you with a brief letter on certain supposed appearances and forms of lightning, respecting which the judgment of even good observers is often in error.

When, after a serene sky, or one that is not overcast, thunder-clouds form in the distance, the observer sees the clouds and the illumination of the lightning displayed before him as a magnificent picture; and what he often takes to be forked lightning (*i. e.* the actual flash, and not a reflexion of it), appears to run through the clouds in the most beautiful manner. This was the case on that evening to those who, being in London, observed the storm in the west, about nine o'clock, when the clouds were at a distance of twenty miles or more; and I have very frequently observed the same effect from our southern coasts over the sea. In many of these cases, that which is thought to be the electric discharge is only the illuminated edge of a cloud, beyond and behind which the real discharge occurs. It is in its nature like the bright enlightened edge which a dark well-defined cloud often presents when between the sun and the observer; and even the moon also frequently produces similar appearances. In the case of its production by lightning and distant clouds, the line is so bright by comparison with the previous state of the clouds and sky, so sudden and brief in its existence, so perfectly defined, and of such a form, as to lead every one at the first moment to think it is the lightning itself which appears.

But the forms which this line assumes, being dependent on the forms of the clouds, vary much, and have led to many mistakes about the shape of the lightning flash. Often, when the lightning is supposed to be seen darting from one cloud to another, it is only this illuminated edge which the observer sees. On other occasions, when he was sure he saw it ascend, it was simply this line more brilliant at its upper than at its lower part. Some writers have described curved flashes of lightning, the electric fluid having parted from the clouds, gone obliquely downwards to the sea, and then turned upwards to the clouds again: this effect I have occasionally seen, and have always found it to be merely the illuminated edge of a cloud.

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[1] Lond. and Edinb. Phil. Mag., 1841, vol. xix. p. 104.

I have seen cases of this kind in which the flash appeared to divide in its course, one stream separating into two; and when flashes seen at a distance are supposed to exhibit this rare condition, it is very important the observer should be aware of this very probable cause of deception.

I have also frequently seen, and others with me, a flash having an apparently sensible duration, as if it were a momentary stream, rather than that sudden, brief flash which the electric spark always presents, whose duration even Wheatstone could not appreciate. This I attribute to two or three flashes occurring very suddenly in succession at the same place, or nearly so, and illuminating the same edge of a cloud.

The effect I have described can frequently be easily traced to its cause, and when thus traced best prepares the mind to appreciate the mistakes it may lead, and has led, to in the character, shape and condition of the lightning flash. It often happens at the sea-side, that, after a fine day, clouds will towards evening collect over the sea on the horizon, and lightning will flash about and amongst them, recurring at intervals as short as two or three seconds, for an hour or more together. At such times the observer may think he sees the lightning of a flash; but if he waits till the next illumination, or some future one, takes place, he will perceive that the flash appears a second time in the same place, and with the same form; or perhaps it has travelled a little distance to the left or right, and yet has the same form as before. Sometimes an apparent flash, having the same shape, has occurred three or four times in succession; and sometimes it has happened that a certain shaped flash having appeared in a certain place, other flashes have appeared in other places, then the first has reappeared in its place, and even the others again in their places. Now in all these cases it was simply the illuminated edges of clouds that were seen, and not the real flashes of lightning. These forms frequently exist in the cloud, and yet are not distinguishable till the lightning occurs. It is easy, however, to understand why they are then only developed, for that which appears in the distance to be one dull mass of cloud, distinguishable in figure only at its principal outline, often consists of many subordinate and well-shaped masses, which, when the lightning occurs amongst or beyond them, present forms and lines before unperceived.

The apparent duration, which I before spoke of, is merely a case of very rapidly recurring flashes, and may, by a careful observer, be easily connected with that which I have now proposed as the best test of the nature of the phenomena.

There are some other circumstances which will help to distinguish the effect I have thus endeavoured to describe from the true appearance of the lightning flash, as the apparent thickness, sometimes, of the supposed flash, and its degree of illumination; but I have, I think, said enough to call attention to the point; and, considering how often the philosopher is, in respect to the character of these appearances, obliged to depend upon the report of casual observers, the tendency of whose minds is generally rather to give way to their surprise than to simplify what may seem remarkable, I hope I have not said too much.

I am, Gentlemen, your obedient Servant,

June 22, 1841. M. FARADAY.

On Static Electrical Inductive Action[1].

To R. Phillips, Esq., F.R.S.

DEAR PHILLIPS,

PERHAPS you may think the following experiments worth notice; their value consists in their power to give a very precise and decided idea to the mind respecting certain principles of inductive electrical action, which I find are by many accepted with a degree of doubt or obscurity that takes away much of their importance: they are the expression and proof of certain parts of my view of induction[1]. Let A in the diagram represent an insulated pewter ice-pail ten and a half inches high and seven inches diameter, connected by a wire with a delicate gold-leaf electrometer E, and let C be a round brass ball insulated by a dry thread of white silk, three or four feet in length, so as to remove the influence of the hand holding it from the ice-pail below. Let A be perfectly discharged, then let C be charged at a distance by a machine or Leyden jar, and introduced into A as in the figure. If C be positive, E also will diverge positively; if C be taken away, E will collapse perfectly, the apparatus being in good order. As C enters the vessel A the divergence of E will increase until C is about three inches below the edge of the vessel, and will remain quite steady and unchanged for any greater depression. This shows that at that distance the inductive action of C is entirely exerted upon the interior of A, and not in any degree directly upon external objects. If C be made to touch the bottom of A, *all* its charge is communicated to A; there is no longer any inductive action between C and A, and C, upon being withdrawn and examined, is found perfectly discharged.

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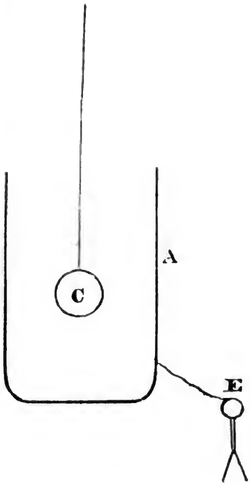
[1] Lond. and Edinb. Phil. Mag., 1843, vol. xxii.



These are all well-known and recognised actions, but being a little varied, the following conclusions may be drawn from them. If C be merely suspended in A, it acts upon it by induction, evolving electricity of its own kind on the outside of A; but if C touch A its electricity is then communicated to it, and the electricity that is afterwards upon the outside of A may be considered as that which was originally upon the carrier C. As this change, however, produces no effect upon the leaves of the electrometer, it proves that the electricity *induced* by C and the electricity *in* C are accurately equal in amount and power.

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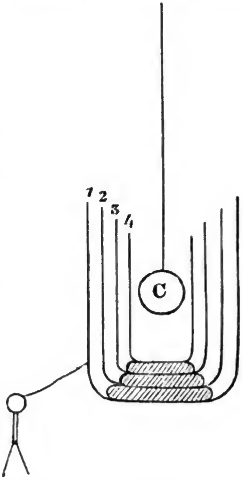
[1] See Experimental Researches, Par. 1295, &c., 1667, &c., and Answer to Dr. Hare, Phil. Mag., 1840, N. S. vol. xvii. p. 56. viii. (or page 264 of this volume.)

Again, if C charged be held equidistant from the bottom and sides of A at one moment, and at another be held as close to the bottom as possible without discharging to A, still the divergence remains absolutely unchanged, showing that whether C acts at a considerable distance or at the very smallest distance, the amount of its force is the same. So also if it be held excentric and near to the side of the ice-pail in one place, so as to make the inductive action take place in lines expressing almost every degree of force in different directions, still the sum of their forces is the same constant quantity as that obtained before; for the leaves alter not. Nothing like expansion or coercion of the electric force appears under these varying circumstances.



I can now describe experiments with many concentric metallic vessels arranged as in the diagram, where four ice-pails are represented insulated from each other by plates of shelllac on which they respectively stand. With this system the charged carrier C acts precisely as with the single vessel, so that the intervention of many conducting plates causes no difference in the amount of inductive effect. If C touch the inside of vessel 4, still the leaves are unchanged. If 4 be taken out by a silk thread, the leaves perfectly collapse; if it be introduced again, they open out to the same degree as before. If 4 and 3 be connected by a wire let down between them by a silk thread, the leaves remain the same, and so they still remain if 3 and 2 be connected by a similar wire; yet all the electricity originally on the carrier and acting at a considerable distance, is now on the outside of 2, and acting through only a small non-conducting space. If at last it be communicated to the outside of 1, still the leaves remain unchanged.

Again, consider the charged carrier C in the centre of the system, the divergence of the electrometer measures its inductive influence; this divergence remains the same whether 1 be there alone, or whether all four vessels be there; whether these vessels be separate as to insulation, or whether 2, 3 and 4 be connected so as to represent a very thick metallic vessel, or whether all four vessels be connected.

Again, if in place of the metallic vessels 2, 3, 4, a thick vessel of shell-lac or of sulphur be introduced, or if any other variation in the character of the substance within the vessel 1 be made, still not the slightest change is by that caused upon the divergence of the leaves.

If in place of one carrier many carriers in different positions are within the inner vessel, there is no interference of one with the other; they act with the same amount of force outwardly as if the electricity were spread uniformly over one carrier, however much the distribution on each carrier may be disturbed by its neighbours. If the charge of one carrier be by contact given to vessel 4 and distributed over it, still the others act through and across it with the same final amount of force; and no state of charge given to any of the vessels 1, 2, 3 or 4, prevents a charged carrier introduced within 4 acting with precisely the same amount of force as if they were uncharged. If pieces of shell-lac, slung by white silk thread and excited, be introduced into the vessel, they act exactly as the metallic carriers, except that their charge cannot be communicated by contact to the metallic vessels.

Thus a certain amount of electricity acting within the centre of the vessel A exerts exactly the same power externally, whether it act by induction through the space between it and A, or whether it be transferred by conduction to A, so as absolutely to destroy the previous induction within. Also, as to the inductive action, whether the space between C and A be filled with air, or with shell-lac or sulphur, having above twice the specific inductive capacity of air; or contain many concentric shells of conducting matter; or be nine-tenths filled with conducting matter, or be metal on one side and shell-lac on the other; or whatever other means be taken to vary the forces, either by variation of distance or substance, or actual charge of the matter in this space, still the amount of action is precisely the same.

Hence if a body be charged, whether it be a particle or a mass, there is nothing about its action which can at all consist with the idea of exaltation or extinction; the amount of force is perfectly definite and unchangeable: or to those who in their minds represent the idea of the electric force by a fluid, there ought to be no notion of the compression or condensation of this fluid within itself, or of its coercibility, as some understand that phrase. The only mode of affecting this force is by connecting it with force of the same kind, either in the same or the contrary direction. If we oppose to it force of the contrary kind, we may *by discharge* neutralize the original force, or we may *without discharge* connect them by the simple laws and principles of static induction; but away from induction, which is *always of the same kind,* there is no other state of the power in a charged body; that is, there is no state of static electric force corresponding to the terms of *simulated* or *disguised* or *latent* electricity away from the ordinary principles of inductive action; nor is there any case where the electricity is *more latent* or *more disguised* than when it exists upon the charged conductor of an electrical machine and is ready to give a powerful spark to any body brought near it.

A curious consideration arises from this perfection of inductive action. Suppose a thin uncharged metallic globe two or three feet in diameter, insulated in the middle of a chamber, and then suppose the space within this globe occupied by myriads of little vesicles or particles charged alike with electricity (or differently), but each insulated from its neighbour and the globe; their inductive power would be such that the outside of the globe would be charged with a force equal to the sum of *all* their forces, and any part of this globe (not charged of itself) would give as long and powerful a spark to a body brought near it as if the electricity of all the particles near and distant were on the surface of the globe itself. If we pass from this consideration to the case of a cloud, then, though we cannot altogether compare the external surface of the cloud to the metallic surface of the globe, yet the previous inductive effects upon the *earth* and its buildings are the same; and when a charged cloud is over the earth, although its electricity may be diffused over every one of its particles, and no important part of the *inductric* charge be accumulated upon its under surface, yet the induction upon the earth will be as strong as if all that portion of force which is directed towards the earth *were* upon that surface; and the state of the earth and its tendency to discharge to the cloud will also be as strong in the former as in the latter case. As to whether lightning-discharge begins first at the cloud or at the earth, that is a matter far more difficult to decide than is usually supposed[1]; theoretical notions would lead me to expect that in most cases, perhaps in all, it begins at the earth. I am,

My dear Phillips, ever yours,

Royal Institution, Feb. 4, 1843. M. Faraday.

A speculation touching Electric Conduction and the Nature of Matter[2].

To Richard Taylor, Esq.

Dear Sir, Royal Institution, Jan. 25, 1844.

Last Friday I opened the weekly evening-meetings here by a subject of which the above was the title, and had no intention of publishing the matter further, but as it involves the consideration and application of a few of those main elements of natural knowledge, facts, I thought an account of its nature and intention might not be unacceptable to you, and would at the same time serve as the record of my opinion and views, as far as they are at present formed.

The view of the atomic constitution of matter which I think is most prevalent, is that which considers the atom as a something material having a certain volume, upon which those powers were impressed at the creation, which have given it, from that time to the present, the capability of constituting, when many atoms are congregated together into groups, the different substances whose effects and properties we observe. These, though grouped and held together by their powers, do not touch each other, but have intervening space, otherwise pressure or cold could not make a body contract into a smaller bulk, nor heat or tension make it larger; in liquids these atoms or particles are free to move about one another, and in vapours or gases they are also present, but removed very much further apart, though still related to each other by their powers.

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[1] Experimental Researches, Par. 1370, 1410, 1484.

[2] Lond. and Edinb. Phil. Mag., 1844, vol. xxiv. p. 136.

The atomic doctrine is greatly used one way or another in this, our day, for the interpretation of phenomena, especially those of crystallography and chemistry, and is not so carefully distinguished from the facts, but that it often appears to him who stands in the position of student, as a statement of the facts themselves, though it is at best but an assumption; of the truth of which we can assert nothing, whatever we may say or think of its probability. The word atom, which can never be used without involving much that is purely hypothetical, is often *intended* to be used to express a simple fact; but good as the intention is, I have not yet found a mind that did habitually separate it from its accompanying temptations; and there can be no doubt that the words definite proportions, equivalents, primes, &c., which did and do express fully all the *facts* of what is usually called the atomic theory in chemistry, were dismissed because they were not expressive enough, and did not say all that was in the mind of him who used the word atom in their stead; they did not express the hypothesis as well as the fact.

But it is always safe and philosophic to distinguish, as much as is in our power, fact from theory; the experience of past ages is sufficient to show us the wisdom of such a course; and considering the constant tendency of the mind to rest on an assumption, and, when it answers every present purpose, to forget that it is an assumption, we ought to remember that it, in such cases, becomes a prejudice, and inevitably interferes, more or less, with a clear-sighted judgment. I cannot doubt but that he who, as a wise philosopher, has most power of penetrating the secrets of nature, and guessing by hypothesis at her mode of working, will also be most careful, for his own safe progress and that of others, to distinguish that knowledge which consists of assumption, by which I mean theory and hypothesis, from that which is the knowledge of facts and laws; never raising the former to the dignity or authority of the latter, nor confusing the latter more than is inevitable with the former.

Light and electricity are two great and searching investigators of the molecular structure of bodies, and it was whilst considering the probable nature of conduction and insulation in bodies not decomposable by the electricity to which they were subject, and the relation of electricity to space contemplated as void of that which by the atomists is called matter, that considerations something like those which follow were presented to my mind.

If the view of the constitution of matter already referred to be assumed to be correct, and I may be allowed to speak of the particles of matter and of the space between them (in water, or in the vapour of water for instance) as two different things, then space must be taken as the only continuous part, for the particles are considered as separated by space from each other. Space will permeate all masses of matter in every direction like a net, except that in place of meshes it will form cells, isolating each atom from its neighbours, and itself only being continuous.

Then take the case of a piece of shell-lac, a non-conductor, and it would appear at once from such a view of its atomic constitution that space is an insulator, for if it were a conductor the shell-lac could not insulate, whatever might be the relation as to conducting power of its material atoms; the space would be like a fine metallic web penetrating it in every direction, just as we may imagine of a heap of siliceous sand having all its pores filled with water; or as we may consider of a stick of black wax, which, though it contains an infinity of particles of conducting charcoal diffused through every part of it, cannot conduct, because a non-conducting body (a resin) intervenes and separates them one from another, like the supposed space in the lac.

Next take the case of a metal, platinum or potassium, constituted, according to the atomic theory, in the same manner. The metal is a conductor; but how can this be, except space be a conductor? for it is the only continuous part of the metal, and the atoms not only do not touch (by the theory), but as we shall see presently, must be assumed to be a considerable way apart. Space therefore must be a conductor, or else the metals could not conduct, but would be in the situation of the black sealing-wax referred to a little while ago.

But if space be a conductor, how then can shell-lac, sulphur, &c. insulate? for space permeates them in every direction. Or if space be an insulator, how can a metal or other similar body conduct?

It would seem, therefore, that in accepting the ordinary atomic theory, space may be proved to be a non-conductor in non-conducting bodies, and a conductor in conducting bodies, but the reasoning ends in this, a subversion of that theory altogether; for if space be an insulator it cannot exist in conducting bodies, and if it be a conductor it cannot exist in insulating bodies. Any ground of reasoning which tends to such conclusions as these must in itself be false.

In connexion with such conclusions we may consider shortly what are the probabilities that present themselves to the mind, if the extension of the atomic theory which chemists have imagined, be applied in conjunction with the conducting powers of metals. If the specific gravity of the metals be divided by the atomic numbers, it gives us the number of atoms, upon the hypothesis, in equal bulks of the metals. In the following table the first column of figures expresses nearly the number of atoms in, and the second column of figures the conducting power of, equal volumes of the metals named.

Atoms.		Conducting power.
1·00	gold	6·00
1·00	silver.	4·66
1·12	lead	0·52
1·30	tin	1·00
2·20	platinum	1·04
2·27	zinc	1·80
2·87	copper	6·33
2·90	iron	1·00

So here iron, which contains the greatest number of atoms in a given bulk, is the worst conductor excepting one; gold, which contains the fewest, is nearly the best conductor. Not that these conditions are in inverse proportions, for copper, which contains nearly as many atoms as iron, conducts better still than gold, and with above six times the power of iron. Lead, which contains more atoms than gold, has only about one-twelfth of its conducting power; lead, which is much heavier than tin and much lighter than platina, has only half the conducting power of either of these metals. And all this happens amongst substances which we are bound to consider, at present, as elementary or simple. Whichever way we consider the particles of matter and the space between them, and examine the assumed constitution of matter by this table, the results are full of perplexity.

Now let us take the case of potassium, a compact metallic substance with excellent conducting powers, its oxide or hydrate a non-conductor; it will supply us with some facts having very important bearings on the assumed atomic construction of matter.

When potassium is oxidized an atom of it combines with an atom of oxygen to form an atom of potassa, and an atom of potassa combines with an atom of water, consisting of two atoms of oxygen and hydrogen, to form an atom of hydrate of potassa, so that an atom of hydrate of potassa contains four elementary atoms. The specific gravity of potassium is 0·865, and its atomic weight 40; the specific gravity of cast hydrate of potassa, in such state of purity as I could obtain it, I found to be nearly 2, its atomic weight 57. From these, which may be taken as facts, the following strange conclusions flow. A piece of potassium contains less potassium than an equal piece of the potash formed by it and oxygen. We may cast into potassium oxygen atom for atom, and then again both oxygen and hydrogen in a twofold number of atoms, and yet, with all these additions, the matter shall become less and less, until it is not two-thirds of its original volume. If a given bulk of potassium contains 45 atoms, the same bulk of hydrate of potassa contains 70 atoms nearly *of the metal potassium*, and besides that, 210 atoms more of oxygen and hydrogen. In dealing with assumptions I must assume a little more for the sake of making any kind of statement; let me therefore assume that in the hydrate of potassa the atoms are all of one size and nearly touching each other, and that in a cubic inch of that substance there are 2800 elementary atoms of potassium, oxygen and hydrogen; take away 2100 atoms of oxygen and hydrogen, and the 700 atoms of potassium remaining will swell into more than a cubic inch and a half, and if we diminish the number until only those containable in a cubic inch remain, we shall have 430, or thereabout. So a space which can contain 2800 atoms, and amongst them 700 of potassium itself, is found to be entirely filled by 430 atoms of potassium as they exist in the ordinary state of that metal. Surely then, under the suppositions of the atomic theory, the atoms of potassium must be very far apart in the metal, *i. e.* there must be much more of space than of matter in that body: yet it is an excellent conductor, and so space must be a conductor; but then what becomes of shell-lac, sulphur, and all the insulators? for space must also by the theory exist in them.

Again, the volume which will contain 430 atoms of potassium, and nothing else, whilst in the state of metal, will, when that potassium is converted into nitre, contain very nearly the same number of atoms of potassium, *i. e.* 416, and also then seven times as many, or 2912 atoms of nitrogen and oxygen besides. In carbonate of potassa the space which will contain only the 430 atoms of potassium as metal, being entirely filled by it, will, after the conversion, contain 256 atoms more of potassium, making 686 atoms of that metal, and, in addition 2744 atoms of oxygen and carbon.

These and similar considerations might be extended through compounds of sodium and other bodies with results equally striking, and indeed still more so, when the relations of one substance, as oxygen or sulphur, with different bodies are brought into comparison.

I am not ignorant that the mind is most powerfully drawn by the phenomena of crystallization, chemistry and physics generally, to the acknowledgement of centres of force. I feel myself constrained, for the present hypothetically, to admit them, and cannot do without them, but I feel great difficulty in the conception of atoms of matter which in solids, fluids and vapours are supposed to be more or less apart from each other, with intervening space not occupied by atoms, and perceive great contradictions in the conclusions which flow from such a view.

If we must assume at all, as indeed in a branch of knowledge like the present we can hardly help it, then the safest course appears to be to assume as little as possible, and in that respect the atoms of Boscovich appear to me to have a great advantage over the more usual notion. His atoms, if I understand aright, are mere centres of forces or powers, not particles of matter, in which the powers themselves reside. If, in the ordinary view of atoms, we call the particle of matter away from the powers *a*, and the system of powers or forces in and around it *m*, then in Boscovich's theory *a* disappears, or is a mere mathematical point, whilst in the usual notion it is a little unchangeable, impenetrable piece of matter, and *m* is an atmosphere of force grouped around it.

In many of the hypothetical uses made of atoms, as in crystallography, chemistry, magnetism, &c., this difference in the assumption makes little or no alteration in the results, but in other cases, as of electric conduction, the nature of light, the manner in which bodies combine to produce compounds, the effects of forces, as heat or electricity, upon matter, the difference will be very great.

Thus, referring back to potassium, in which as a metal the atoms must, as we have seen, be, according to the usual view, very far apart from each other, how can we for a moment imagine that its conducting property belongs to it, any otherwise than as a consequence of the properties of the space, or as I have called it above, the *m*? so also its other properties in regard to light or magnetism, or solidity, or hardness, or specific gravity, must belong to it, in consequence of the properties or forces of the *m*, not those of the *a*, which, without the forces, is conceived of as having no powers. But then surely the *m* is the *matter* of the potassium, for where is there the least ground (except in a gratuitous assumption) for imagining a difference in kind between the nature of that space midway between the centres of two contiguous atoms and any other spot between these centres? a difference in degree, or even in the nature of the power consistent with the law of continuity, I can admit, but the difference between a supposed little hard particle and the powers around it I cannot imagine.

To my mind, therefore, the *a* or nucleus vanishes, and the substance consists of the powers or *m*; and indeed what notion can we form of the nucleus independent of its powers? all our perception and knowledge of the atom, and even our fancy, is limited to ideas of its powers: what thought remains on which to hang the imagination of an *a* independent of the acknowledged forces? A mind just entering on the subject may consider it difficult to think of the powers of matter independent of a separate something to be called *the matter*, but it is certainly far more difficult, and indeed impossible, to think of or imagine that *matter* independent of the powers. Now the powers we know and recognize in every phenomenon of the creation, the abstract matter in none; why then assume the existence of that of which we are ignorant, which we cannot conceive, and for which there is no philosophical necessity?

Before concluding these speculations I will refer to a few of the important differences between the assumption of atoms consisting merely of centres of force, like those of Boscovich, and that other assumption of molecules of something specially material, having powers attached in and around them.

With the latter atoms a mass of matter consists of atoms and intervening space, with the former atoms matter is everywhere present, and there is no intervening space unoccupied by it. In gases the atoms touch each other just as truly as in solids. In this respect the atoms of water touch each other whether that substance be in the form of ice, water or steam; no mere intervening space is present. Doubtless the centres of force vary in their distance one from another, but that which is truly the matter of one atom touches the matter of its neighbours.

Hence matter will be *continuous* throughout, and in considering a mass of it we have not to suppose a distinction between its atoms and any intervening space. The powers around the centres give these centres the properties of atoms of matter; and these powers again, when many centres by their conjoint forces are grouped into a mass, give to every part of that mass the properties of matter. In such a view all the contradiction resulting from the consideration of electric insulation and conduction disappears.

The atoms may be conceived of as highly *elastic*, instead of being supposed excessively hard and unalterable in form; the mere compression of a bladder of air between the hands can alter their size a little; and the experiments of Cagniard de la Tour carry on this change in size until the difference in bulk at one time and another may be made several hundred times. Such is also the case when a solid or a fluid body is converted into vapour.

With regard also to the *shape* of the atoms, and, according to the ordinary assumption, its definite and unalterable character, another view must now be taken of it. An atom by itself might be conceived of as spherical, or spheroidal, or where many were touching in all directions, the form might be thought of, as a dodecahedron, for any one would be surrounded by and bear against twelve others, on different sides. But if an atom be conceived to be a centre of power, that which is ordinarily referred to under the term *shape* would now be referred to the disposition and relative intensity of the forces. The power arranged in and around a centre might be uniform in arrangement and intensity in every direction outwards from that centre, and then a section of equal intensity of force through the radii would be a sphere; or the law of decrease of force from the centre outwards might vary in different directions, and then the section of equal intensity might be an oblate or oblong spheroid, or have other forms; or the forces might be disposed so as to make the atom polar; or they might circulate around it equatorially or otherwise, after the manner of imagined magnetic atoms. In fact nothing can be supposed of the disposition of forces in or about a solid nucleus of matter, which cannot be equally conceived with respect to a centre.

In the view of matter now sustained as the lesser assumption, matter and the atoms of matter would be mutually penetrable. As regards the mutual penetrability of matter, one would think that the facts respecting potassium and its compounds, already described, would be enough to prove that point to a mind which accepts a fact for a fact, and is not obstructed in its judgement by preconceived notions. With respect to the mutual penetrability of the atoms, it seems to me to present in many points of view a more beautiful, yet equally probable and philosophic idea of the constitution of bodies than the other hypotheses, especially in the case of chemical combination. If we suppose an atom of oxygen and an atom of potassium about to combine and produce potash, the hypothesis of solid unchangeable impenetrable atoms places these two particles side by side in a position easily, because mechanically, imagined, and not unfrequently represented; but if these two atoms be centres of power they will mutually penetrate to the very centres, thus forming one atom or molecule with powers, either uniformly around it or arranged as the resultant of the powers of the two constituent atoms; and the manner in which two or many centres of force may in this way combine, and afterwards, under the dominion of stronger forces, separate again, may in some degree be illustrated by the beautiful case of the conjunction of two sea waves of different velocities into one, their perfect union for a time, and final separation into the constituent waves, considered, I think, at the meeting of the British Association at Liverpool. It does not of course follow, from this view, that the centres shall always coincide; that will depend upon the relative disposition of the powers of each atom.

The view now stated of the constitution of matter would seem to involve necessarily the conclusion that matter fills all space, or, at least, all space to which gravitation extends (including the sun and its system); for gravitation is a property of matter dependent on a certain force, and it is this force which constitutes the matter. In that view matter is not merely mutually penetrable, but each atom extends, so to say, throughout the whole of the solar system, yet always retaining its own centre of force. This, at first sight, seems to fall in very harmoniously with Mossotti's mathematical investigations and reference of the phenomena of electricity, cohesion, gravitation, &c. to one force in matter; and also again with the old adage, "matter cannot act where it is not." But it is no part of my intention to enter into such considerations as these, or what the bearings of this hypothesis would be on the theory of light and the supposed æther. My desire has been rather to bring certain facts from electrical conduction and chemical combination to bear strongly upon our views regarding the nature of atoms and matter, and so to assist in distinguishing in natural philosophy our real knowledge, *i. e.* the knowledge of facts and laws, from that, which, though it has the form of knowledge, may, from its including so much that is mere assumption, be the very reverse.

I am, my dear Sir, yours, &c.,
MICHAEL FARADAY.

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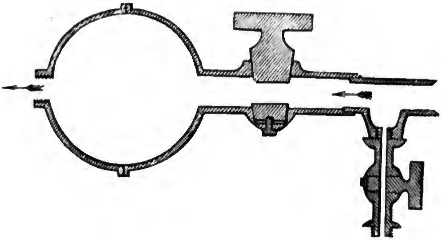

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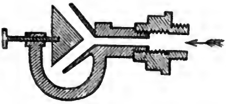

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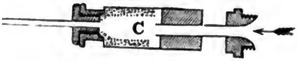

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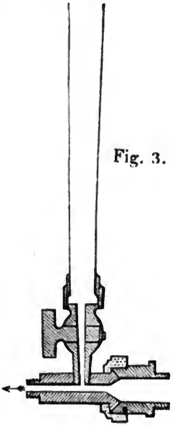

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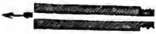

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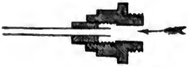

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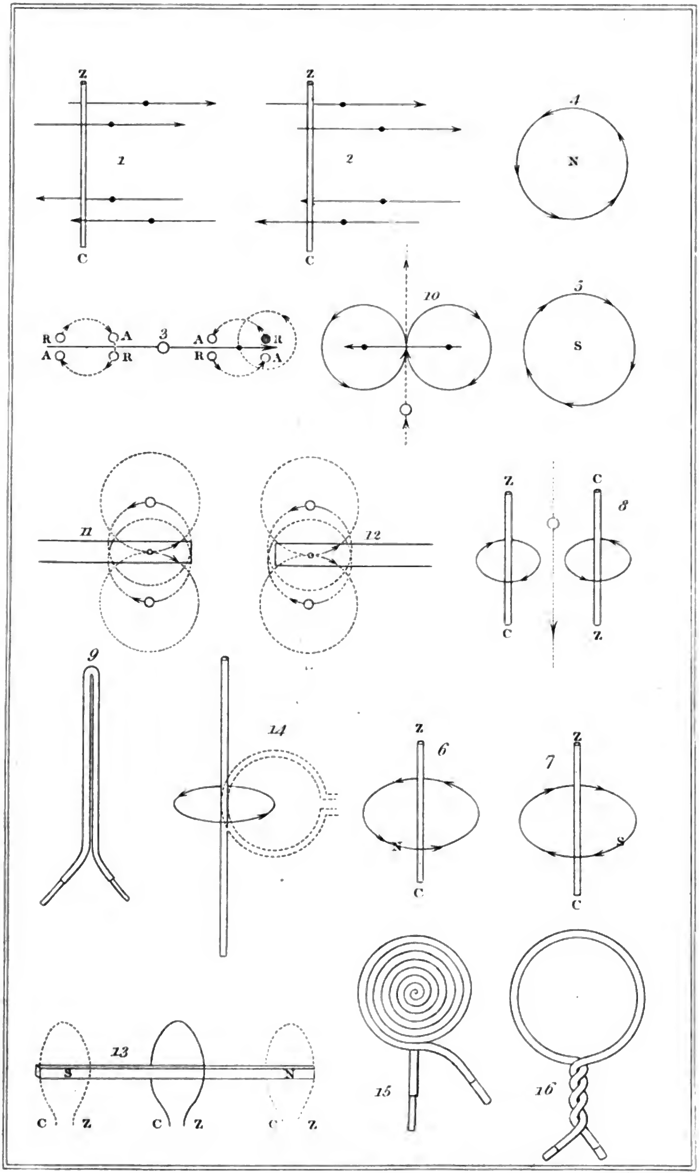

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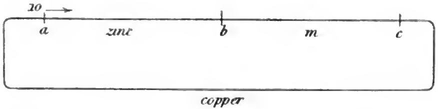

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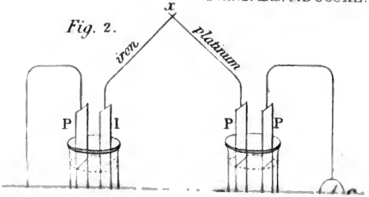

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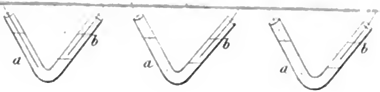

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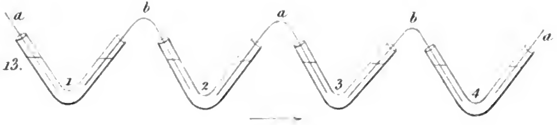

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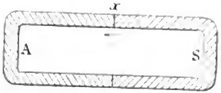

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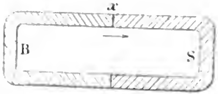

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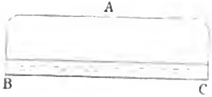

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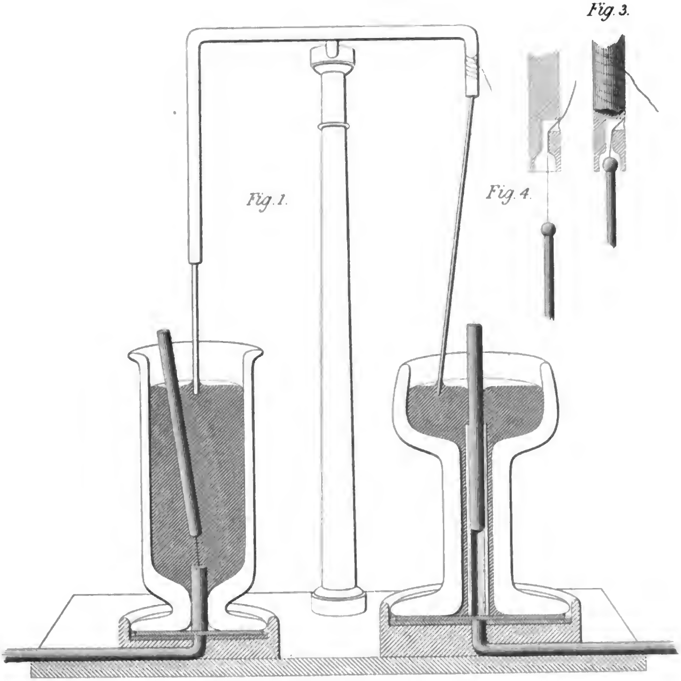

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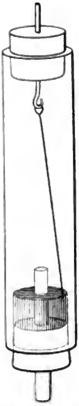

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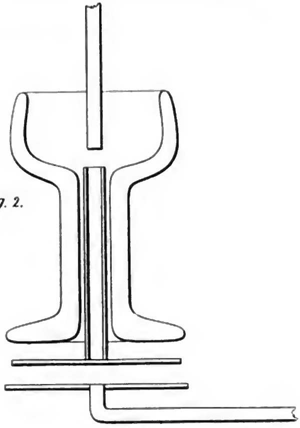

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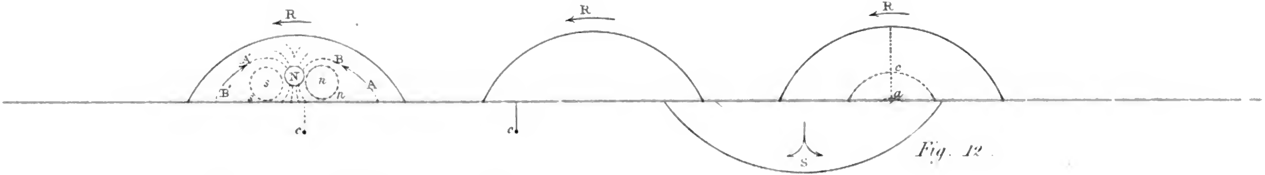

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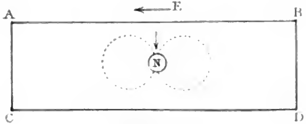

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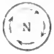

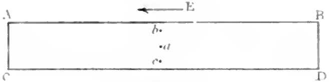

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